Worker Voice and Shared Governance: Evidence from a Reform in Finland*

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Abstract

We estimate the effects of a 1991 reform in Finland that introduced worker representation on board seats or in management bodies of firms with more than 150 workers. In regression-discontinuity specifications, we find that worker representation *increases* investment and the capital stock by 10-22%. We find no analogous effects at the policy discontinuity in the pre-reform period. Worker representation raises overall wages by about 5%. We find no effects on efficiency (TFP), profitability, or dividend payouts. Overall, our evidence stands in sharp contrast to the disinvestment predictions of hold-up theories.

Text

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1 Introduction

We study the firm-level effects of shared governance—granting workers formal decision rights in their firm, specifically granting workers the right to appoint representatives to company boards or management bodies alongside owners. We focus on the influential hold-up view, which provides a powerful caveat to any institutions that raise labor's bargaining power: such institutions might lead to higher wages at the expense of capital income, thereby depressing capitalists' willingness to invest, ultimately leading to a decline in the capital stock in the firm and the economy (Grout, 1984; Jensen and Meckling, 1979; Jäger, Schoefer, and Heining, 2019).[1] By this view, shared governance essentially acts as a distortionary tax on capital income.

Our empirical design compares variation across firms with and without shared governance in Finland arising from a policy discontinuity in firm size introduced by a 1991 reform. In firms with 150 or more workers, employees have a right to nominate worker representatives to participate in firm-level decision making. The rules affect all for-profit firms. No such right exists for workers in firms below that threshold. In firms meeting the policy threshold, the exact form of worker participation is in most cases negotiated through an agreement between workers and the firm. Absent an agreement, workers have a statutory right to nominate 20% of the representatives in either the board of directors, the board of supervisors, or the management body – with the specific body of representation, in turn, selected by the firm. Our reduced-form specifications do not condition on specific arrangements, and encompass three effects: the default statutory 20% representation (in turn of either of the three types), negotiated arrangements, and—even in firms without take-up—the threat arising from the unexercised right to representation.

We draw on firm and worker administrative data sets from tax and other sources to compare firms above and below the 150-employee threshold while the policy was active. Specifically, our main design is a regression discontinuity (RD) design. We also consider placebo specifications, counterfactually assuming that the policy discontinuity at 150 existed already in pre-periods, as well as at a placebo threshold of 100 employees. We validate our RD design by ruling out that firms strategically distort employment below the cutoff in a McCrary (2008) test, and assess the continuity of industry composition at the threshold. Importantly, we find no bunching of firms below the 150 employee threshold, indicating that firms do not appear to evade falling under the shared governance law

[1] For example, Jensen and Meckling (1979) describe the hold-up channel of shared governance as follows: "[T]he workers will begin 'eating it [the firm] up' by transforming the assets of the firm into consumption or personal assets. [...] It will become difficult for the firm to obtain capital in the private capital markets. [...] The result of this process will be a significant reduction in the country's capital stock, increased unemployment, reduced labor income, and an overall reduction in output and welfare."

– a property necessary for the validity of our identification strategy, as well as a result interesting in its own right.

Our first main result is that we find *positive* effects of shared governance on capital formation. Investment expenditures increase by about 25% and total assets by around 16%. These effects are driven by increases in fixed assets, which encompass investment goods such as buildings, machines, and equipment, rather than current (or financial) assets. Hence, we find that the disinvestment prediction of the hold-up view is not borne out in the Finnish context for this institution of shared governance.

Our empirical findings against disinvestment effects is consistent with evidence for positive capital effects of worker representation on company boards in Germany (Jäger, Schoefer, and Heining, 2019) and with observational evidence from comparing establishments with and without works councils (Addison et al., 2007).[2] Similarly, our results are also in line with Redeker (2019) who compares corporate savings of listed firms in Germany with one-third vs. parity board representation. At a broader level, our results are also consistent with the absence of hold-up patterns in firm-level dynamics in Italy (Card, Devicienti, and Maida, 2014).

In contrast to the hold-up view, richer views of industrial relations or theories of the second best might account for our findings. For instance, a richer model of shared governance with workers participating not only in wage setting but also in input decisions, such as over capital (Manning, 1987; Jäger, Schoefer, and Heining, 2019), delivers predictions consistent with our main findings. In fact, the Finnish institution comes with a broad range of company outcomes over which worker representatives may have direct influence, including investment decisions directly, but also by controlling the executives. Models of efficient, joint bargaining can predict investment to increase if shared governance induces such a regime shift (Crawford, 1988). Alternatively, shared governance may ameliorate other frictions such as coordination issues and imperfect information sharing (Freeman and Medoff, 1985; Freeman and Lazear, 1995), support implicit contracts or long-term interactions (Malcomson, 1983; Hogan, 2001). Our evidence does not distinguish these views. However, we cannot detect effects on total factor productivity (finding positive but not

[2]Jäger, Schoefer, and Heining (2019) is most closely related to our study, in that it aims to estimate the causal effects of shared governance on firm outcomes, with a focus on capital formation and the hold-up prediction. That paper also finds positive investment effects, studying a cohort-based reform in Germany that abolished shared governance in small firms but locked older cohorts into their pre-existing regime. The sample are stock corporations, and the institution is one-third supervisory board seat quotas for worker-elected representatives. By contract, the present paper studies a reform that imposes the institution onto incumbent firms, and the Finnish institution provides for more flexible implementation (permitting management involvement or other local arrangements besides board representation), and occurs in the context of a largely unitary board structure. The worker representatives are statutorily set to be 20% in the Finnish context, compared to the variation of 33% in the German reform.

precisely estimated output effects), hence ruling out large effects on worker involvement to make firms more efficient.

Our second focus is on the consequences of shared governance for capital income and labor income. These outcomes are the transmission mechanisms in the theoretical views of hold-up: workers' are believed to bargain for a larger share of the firms' value added, leading to higher wages. Anticipating the lower capital share, owners will cut back on investment. Moreover, the theoretical debate as to the voluntary adoption of shared governance centers around the idea that it would hurt profits (Jensen and Meckling, 1979; Freeman and Lazear, 1995), an outcome we directly study.

On the labor income side, we find that shared governance raises firms' wage policies by moderate amounts. First, we document a 9% increase of total labor costs and a 5% increase in log wages. further identify firms' wage policies by implementing Abowd, Kramarz, and Margolis (1999) specifications, hence netting out workforce composition. Here, we also find that shared governance firms appear to attract workers with high earnings potential, such that raw wage effects are larger (yet this selection would not reflect actual pay premia). This wage effect does not arise from larger rent sharing, as we do not detect higher firm-level elasticity of worker wages to productivity shifts in shared governance firms. Perhaps this finding helps explain the absence of disinvestment effects, as hold-up requires workers' *share* of value added to increase, whereas unconditional wage increases would not distort marginal investment incentives. We further document evidence consistent with wage compression and our point estimates suggest a 3.5% reduction in the ratio of wages of workers at the 75th percentile to the ones of those at the 25th percentile. In addition, our point estimates suggest a negative effect in the share of payroll going to executives.

On the capital side, we find no change in the capital share of value added as well as an increase in capital income (cash flow, i.e., value added minus labor costs). In large part, this positive effect reflects the increased capital intensity of production. We further find a zero effect on net income per equity.

Finally, our data also permit us to directly study dividend payouts to firm owners. We can therefore directly study the additional investment that may in fact arise from worker representatives locking cash flow inside the firm in form of retained earnings, as could be consistent with, e.g., empire building on the part of managers colluding with workers. Here, we find positive point estimates close to zero, allowing us to rule out even small negative effects on the income received by shareholders. Hence, overall, we find no evidence that shared governance measurably hurts capitalists.

We dedicate our conclusion, Section 6, to reflecting on the particular institutional context, the external validity and implications of our findings. In Section 2, we describe

the reform and the Finnish codetermination and wage setting institutions. Section 3.1 presents the research design and the data. We present the results on production outcomes in Section 4.1, with a particular focus on capital and investment. Section 5 contains the results on labor and capital incomes.

2 Institutional Context and the 1991 Reform

We provide an overview of corporate governance, the reform that introduced firm-level codetermination, and wage setting institutions in Finland.

Corporate Governance in Finland Finnish companies follow the Nordic board model and overwhelmingly feature a single-tier board structure with a board of directors elected by the general meeting of the shareholders (see, Lekvall et al., 2014, Appendix B, for a detailed overview of corporate governance in Finland).[3] We illustrate this governance structure in Figure 1 Panel (a). The board of directors is responsible for determining the strategy of the firm as well as for appointing, dismissing at will, and overseeing the managing director, who runs the firm on a day-to-day level and is *de jure* not an employee of the firm. The Finnish Corporate Governance Code advises that the majority of directors ought to be independent. In practice, most boards are exclusively comprised of non-executive directors and, in 2013, only 15% of listed firms had their managing director on the board of directors (Lekvall et al., 2014). Executives, including the managing director, must follow and implement instructions from the board of directors (Ringe, 2016). The general meeting of the shareholders sets the compensation for directors; the board of directors or a committee comprised of non-executive directors sets the compensation for the managing director.

1991 Reform: Introduction of Firm-Level Codetermination Up until the 1991, workers had not held an active role in firm-level decision-making, even though workers in most firms had shop-floor representation through union representatives with some information and consultation rights. A 1991 reform introduced shared governance and employee participation in firms with at least 150 employees. The law (725/1990) was passed in 1990 by a coalition government between the center-right party (KOK) and the Social Democratic Party and two smaller parties. The law was the result of a political compromise, with employer associations opposing the statutory rights eventually given to employees, while

[3]Firms can also choose to adopt a two-tier structure with a supervisory board, as in, e.g., Germany, even though very few do so.

Social Democrats called for a lower threshold of 30 employees (Marttila, 2016, p. 224). The law allowed for shared governance by mutual agreement starting on January 1, 1991, and then installed the statutory right to participation in firm governance starting with the first general meeting held after July 1, 1992. The law has been in place without major changes since it was first passed.

Shared Governance Following 1991 Reform As a consequence of the 1991 reform, workers in firms with at least 150 employees have a right to participate in the governance of their firms and to be involved in business and financial decisions.[4] The typical form of representation is through an agreement between the firm and worker representatives of at least two employee groups (manual, non-manual and managerial workers) representing a majority of employees. If no agreement is reached, workers have a statutory right to appoint representatives to the board of directors (or the supervisory board, in the less common dual board structure) or the management group, with the firm choosing between these two options. We illustrate representation on the board of directors as well as in the management group in Figure 1 Panel (b). Statutorily, between one and four worker-elected representatives can be elected and can make up 20% of the respective body (although, by agreement rather than default, firms can expand this share voluntarily). By law, worker representatives must be employees of the firm (rather than being outside union representatives) and have the same rights and duties as other non-worker representatives. Exceptions are the selection and dismissal of, and compensation setting for management, and workforce wage setting and other employment-related matters such as strikes.

In a 2019 survey among union members conducted by the trade union federation (Teollisuuden palkansaajat, 2019), 50% of workers in firms with more than 150 employees reported that their firms featured formal forms of shared governance (see visualization in Figure 1 Panel (c)). Among those, 27% followed statutory provisions through the law rather than by agreement. Among firms with shared governance, 40% featured worker representatives on the management team, 26% on the board of directors, and 9% on the supervisory board. Other forms of representation that were reported include advisory boards or regular meetings between top-level management and worker representatives.

Survey and qualitative evidence suggests that worker representatives may influence a variety of firm outcomes. According to a survey among 203 worker representatives conducted by the Finnish Metalworkers' Union in 2001, a substantial share of representatives on the board of directors or the management team report exerting influence over a variety

[4]In principle, as in any context, before the reform (and after the reform, if below the cutoff) firms could have voluntarily employed shared governance or installed formal or informal worker voice mechanisms including board representation of workers chosen by shareholders, for example.

of decision-making processes in their respective firms (Sairo, 2001). In particular, 32% of surveyed management team representatives and 44% of board representatives reported having exerted moderate or high levels of influence over their firms' investment decisions. In an interview conducted by Michael Gold and reported in (Gold, Kluge, and Conchon, 2010), a worker board representative of a shipbuilding company in Turku, Finland, describes concrete examples of how he acts as an intermediary between the board and the workforce and exerts influence over decisions regarding production, workplace safety and environment, or outsourcing (see excerpts in Appendix XX). He also describes that while he does not directly participate in worker pay negotiations his knowledge of the firm's economic situation acts both as a constraint on communication by management in negotiations with the workforce as well as an input into the negotiation strategy of the shop stewards (see, e.g., Freeman and Lazear, 1995, for how such a reduction in information asymmetry may increase efficiency).

Wage Setting in Finland The Finnish labor market features a high coverage rate of collective bargaining that leaves substantial room for firm-specific wage setting. Unions and employer associations negotiate collective agreements that mandate wage floors at the occupation and job level. The wage floors are rarely directly binding as most employees receive pay premia above the floors (Uusitalo and Vartiainen, 2009). Firms can also deviate from wage increases negotiated in a collective agreement, and can even negotiate pay cuts with consent of the local bargaining parties. Dickens et al. (2007) report low downward nominal (but high real) wage rigidity in Finland in an international comparison. Firm-specific pay policies, with profit-sharing arrangements and links between wages and productivity, have become increasingly common since the 1990s (Uusitalo and Vartiainen, 2009). Similarly, performance pay has become more common, with more than half of white-collar and about a third of blue-collar workers receiving some form of performance pay in 2000 (Snellman, Uusitalo, and Vartiainen, 2003). In Section 5, we directly estimate specifications relating changes in wages and productivity (value added per worker) at the firm level (see also Figure **??**, panel (d)). On average, we find a rent-sharing elasticity of about 0.05, i.e. within the range of estimates surveyed in the literature (Card et al., 2018; Jäger et al., 2019), and consistent with idiosyncratic productivity shifts affecting wages. In addition, we estimate Abowd, Kramarz, and Margolis (1999) specifications and find substantial between-firm variation in pay policies, accounting for a substantial share of the overall worker-level variation in wages, consistent with substantial scope for firm-level

wage setting.[5]

3 Research Design and Data

Here, we describe our main research design, which exploits the policy discontinuity at the 150 employee threshold in the post-reform period. We also describe placebo discontinuity specifications in the pre-reform period as well a placebo threshold after the reform. Section 3.2 describes our data.

3.1 Regression Discontinuity Design

Our main research design estimates the causal effects of worker representation in firm governance in a regression discontinuity design comparing firms above and below the 150-employee threshold:

$$y_{it} = \alpha + \beta_1 \underbrace{\mathbb{1}[N_{it} \geq 150]}_{\text{Worker Rep.}} + \beta_2 \cdot (N_{it} - 150) + \beta_3 \mathbb{1}[N_{it} \geq 150](N_{it} - 150) + \gamma_t + \nu_{J(i)} + \epsilon_{it}, \quad (1)$$

where y_{it} denotes the outcome of firm i in year t. The running variable N_{it} corresponds to the employment concept relevant to the codetermination law. That is, it counts all employees with more than 90 days of employment and positive earnings in a given year; we do not count short temporary job contracts such as seasonal workers.

Importantly, there are no other policy discontinuities, such as tax incentives or administrative burdens, that kick in at the 150 employee threshold. The coefficient of interest is β_1 and captures the effect of the right to worker representation. To increase precision, our specification also includes year effects, γ_t, and industry effects, $\nu_{J(i)}$. We separately show that the industry distribution is smooth at the policy discontinuity by using predicted outcomes based on leave-out means at the industry levels as outcomes that we report in each specification. Finally, we winsorize outcomes y_{it} at the 1% level.

Bandwidth Choice and Inference We estimate linear and quadratic specifications of (1). Our main specification uses the bandwidth choice procedure in Calonico, Cattaneo, and Titiunik (2014), and we also report results for specifications with a constant bandwidth

[5]We plot the standard deviation of log wages and AKM firm effects over time in Appendix Figure A.1. We find an increase in the standard deviation of wages during the 1990s. The standard deviation of AKM firm effects is about 1/4 that of overall wage dispersion.

of 50 employees around the threshold to maintain comparability across outcomes. We cluster standard errors at the firm level.

McCrary Test We implement a McCrary (2008) test for discontinuity of the density of firms at the 150 employee threshold and plot the density in Figure 2 Panel (a). The corresponding McCrary (2008) test does not reject continuity of the density at 150 employees ($p = 0.498$).[6]

The absence of bunching to the left of the 150 threshold is already a substantial result in and of itself as it shows that firms do not manipulate their size to avoid falling under the shared governance policy (thereby differing from, e.g., evidence for size-dependent regulations to distort firm size in France and Finland Garicano, Lelarge, and Van Reenen, 2016; Harju, Matikka, and Rauhanen, 2019).

Placebo Specifications We additionally estimate placebo specifications in the pre-reform period when no policy discontinuity existed at the 150 employee threshold.A caveat is that, because the pre-period data only covers a subset of the variables and only goes back to 1988, we have three years of observations in the pre-period; consequently, our pre-period specifications have wider standard errors. We therefore also add a placebo specification for a counterfactual policy discontinuity at 100 employees (following Ganong and Jäger, 2018). Finally, we estimate a placebo specification that uses the leave-out mean at the industry-year level for the respective outcome variable as outcome (e.g., the leave-out mean capital share in the relevant industry-year cell). Comparing the true effects to the ones on this predicted outcome allows us to detect the extent to which the effects we find in our non-placebo specification are driven by compositional changes of firms above or below the policy discontinuity.

3.2 Data

We use several administrative data sources on firm- and worker-level outcomes from Statistics Finland as well as from the Finnish Tax Administration. We CPI-adjust all nominal variables (measured in Finnish markka until 2001 and EUR thereafter) to 2010 EUR.

[6]Since we also implement placebo regression discontinuity specifications in the pre-reform period, we also plot the density in the pre-reform period from 1988 to 1990 in Appendix Figure A.2 ($p = 0.948$ for McCrary (2008) test).

Firm-level Accounting Data (1988 to 2017) Our main dataset, the Financial Statement Data Panel, contains firm-level accounting data from Statistics Finland. The data set is available from 1988 to 2017. The sample is based on Statistics Finland's own survey from 1988 to 1993, including all large enterprises (larger than 100 employees in manufacturing and trade, and larger than 50 employees in construction and road transport) and a sample of smaller firms based on stratified sampling by industry and the size category of personnel. 1994 onward the data cover the universe of firm population.

The variable content of the data set has also a break in 1994. Some of the variables are missing from the sample for the period before 1994, such as investments and dividends. However, many of the standard profit and loss account variables, e.g., net profits and wage bill are available also before 1994. Similarly, balance sheet information, for example, fixed and total assets and various debt categories are available consistently before and after 1994.

Tax and Accounting Data from Finnish Tax Administration (1994 to 2016) We merge on firm-level tax and accounting data from the Finnish Tax Administration, which covers all firms from 1994 to 2016. These data include all relevant variables in taxation such as sales, inputs, investments, depreciation, other deductions and taxable income.

Matched Employer-Employee Data (1987 to 2017) This dataset, the FOLK Employment Relationship Data, contains individual-level information that we match to the firm-level data sets. The data include both employee and employer identifiers and information about the length of employment relationships in days (spells) at the employee-employer level within a year. We first transform this data set to a long format by year. Then using employee identifiers we merge it with a data set (FOLK Basic panel) that includes all Finnish individuals and their background information such as gender, age, education, occupation group and annual total earned income in each year from 1988 to 2017. Finally, we merge all this information to the firm-level accounting and tax data mentioned above using firm identifiers.

Employment Measure The employment concept relevant for the shared governance threshold (and our running variable) is the number of employees excluding temporary and seasonal workers. We construct this analogue from our data on employee-employer job contract pairs. We exclude temporary and seasonal workers from the workforce by dropping all workers with fewer than 91 days of contracted work with a specific employer within a year and workers with no earned income in a a given year. Any mismeasurement will likely attenuate a potential treatment effect in our RD design, which assumes that

we precisely measure the statutory employment concept in the running variable. Also, as our data set is in panel format, we can follow firms over time and identify firms with 150 employees or more in consecutive years. Based on our conversations with legal practitioners, the conditions to trigger the codetermination law are definitively met when a firm meets the 150 employee threshold in two consecutive years. Our running variable for the regression discontinuity design is contemporaneous employment in a given year. We also assess an indicator for having had at least 150 employees in the last two years. In Figure 2 Panel (b), we show that this probability changes discontinuously by 47.2 percentage points (SE 0.018) at the threshold. This indicator is necessarily zero for all firms firms below the threshold in the given year. An instrumental variables interpretation of our findings, using the two-year trigger described by legal practitioners, would thus lead one to roughly double the reduced-form estimates (i.e., scale up by $1/0.472$).

Executive Compensation and Company Board Member Data (1994 to 2018) We draw on individual-level board-member data for all Finnish firms. On top of the individual-level identifier, these data include the status of the board member: CEO, regular board member and substitute member. These data are available from 1994 to 2018. We merge board data to individual-level (FOLK Basic panel) income data to measure executive compensation.

4 Production and Capital Intensity

We present the effects on production, capital intensity in Table 2. Appendix Tables A.3 through A.6 report additional specifications and outcomes .

4.1 Capital and Investment

Our core set of outcomes is investment, the capital stock, and capital intensity of the production process. These variables are of particular interest because the hold-up view of shared governance predicts negative effects on capital formation (Jensen and Meckling, 1979; Jäger, Schoefer, and Heining, 2019).

Investment As a first outcome we study investment (capital expenditure) in Figure 3 Panel (a) and columns (1) through (4) of Table 1. Visually, a clear discontinuity of log investment at the 150 employee threshold of about 0.2 is discernible. In column (1) of Table 1, we find a positive effect of 0.232 (SE 0.094) in the linear specification and 0.240 (SE 0.118) in the quadratic specifications with optimal bandwidth choice. Quantitatively, the

estimates imply that investment is about a quarter high in firms in which workers have a right to participate in governance. We find no evidence for placebo effects at the 100 employee cutoff or for compositional changes as indicated by predicted outcomes shifting (unlike for assets, we cannot study effects on investment in the pre-period as this variable is not recorded then).

A potential concern with the previous specification is that log investment is only defined for strictly positive values. We first check whether the effects shroud or are driven by an extensive margin effect (e.g., if firms are less likely to invest on the extensive margin). In column (2) of Table 1, we instead show that, if anything, firms with shared governance are slightly *more* likely to have strictly positive capital expenditure with a point estimate of 0.014 in the linear (SE 0.015) and quadratic specifications (SE 0.017). To further probe the robustness to the inclusion of zero capital expenditure, we additionally also use an inverse hyperbolic transformation of investment and, due to the lumpy nature of investment, we consider a rolling average over three years. In column (3), we document positive estimates of 0.169 (SE 0.087) and 0.218 (SE 0.102) for the transformed outcome variable. Our conclusions are thus robust to explicitly accounting for zero capital expenditure. We furthermore find no significant effects for predicted outcomes or when assessing estimates at the placebo discontinuity at 100 employees.

Finally, we do not detect any significant effects on the composition of investment between (i) machines and equipment, (ii) buildings and structures, and (iii) software, reported in Appendix Table A.2, suggesting that increases in investment arise across the board.

Capital Stocks We report effects on total assets in Figure 3 Panel (b) and in column (4) of Table 1. The figure shows a sharp and discernible change in total assets of about 0.1 at the policy discontinuity of 150 employees. We report estimation results in column (4) of Table 1 and find estimates of 0.148 (SE 0.065) in the linear specification and 0.146 (SE 0.079) in the quadratic specification. That is, shared governance leads to about a fifteen percent increase in total assets. We find substantially smaller or even negative point estimates of -0.032 (pre-period), 0.037 (placebo discontinuity at 100) and 0.006 (predicted outcomes).

We next decompose the asset effects in Figure 5 and columns (5) and (6) of Table 1 and show that the asset effects are driven by fixed rather than current assets. Fixed assets consist of tangibles (buildings and equipment) and intangibles (e.g., patents or trademarks), which our data do not further separate out. This is the closest measure to productive capital that our data allow us to measure. Here, we again document large positive effects of 0.233 (SE 0.106) and 0.277 (SE 0.123). We also investigate effects on fixed assets in the pre-

period and find substantially smaller or even negative (pre-period) placebo estimates. We also study current assets, such as cash, inventory or accounts receivable, and find a substantially smaller effect and negative point estimates of -0.051 (SE 0.100) and -0.041 (SE 0.113), reported in column (6). The total asset effect we documented in column (4) is thus driven by fixed rather than current assets.

The positive effects on capital formation reject the disinvestment predicted by the hold-up view, implying that it either is quantitatively irrelevant for this specific institution, or that counteracting forces offset holdup by crowding in more investment.

4.2 Scale and Productivity

We next study with effects on production scale, studying output and input measures, including labor inputs. Having studied firm scale, which is also a marker of productivity in many models (see, e.g., Lucas, 1978; Melitz, 2003), we then estimate productivity effects directly.

Output We report effects on log revenue and value added in Table 2. We find positive point estimates of 0.064 (SE 0.046) in the linear specification and 0.063 (0.055) in the quadratic specification. We find comparable positive effects on value added of 0.07 (0.043) and 0.048 (SE 0.053) in the linear and quadratic specifications, respectively. For both outcomes, we find no tantamount positive effects in the pre-period or for predicted outcomes; however, we find sizable (but also not significant) estimates at the placebo discontinuity of 200 employees of about 0.05. Taking the linear specifications for the treatment effects, our estimates allow us to rule out that shared governance reduces revenue and output by more than 0.026 or 0.014, respectively.

We have further investigated whether firms with shared governance outsource fewer production activities (as documented in Jäger, Schoefer, and Heining, 2019, , e.g., for the share of revenue produced in-house), but found no evidence for such a mechanism, as indicated already by the quantitatively similar revenue and value added point estimates. We also report effects on log materials directly in column (3) of Table 2 and who positive but statistically not significant effects of about 0.11.

Tenure We next study tenure in column (4) of Table 2. Tenure or turnover may plausibly be affected by worker voice (Hirschman, 1970). Tenure is also a proxy for worker surplus and job quality capturing job attributes valued by workers beyond wages (which we will study below separately). We find an increase of 2.9 log points (SE 3.4) on tenure in the linear specification and 6.8 log points (SE 4.2) in the quadratic specification. Since our matched

employer-employee data only go back to 1988, we have also studied effects from 2005 to 2016, when right-censoring of tenure ought to matter less, and find slightly larger effects of 0.038 (0.047) in the linear specification and 0.105 (SE 0.059), i.e., statistically marginally significant, in the quadratic specification.[7] Our results are thus consistent with small or moderate positive effects on tenure but we cannot reject a zero effect at the 5% level in any specification.

Education and Occupation Groups We next study the education and occupation structure and find no compositional shifts. Since we consider a total of eight different groups, we report results in Appendix Tables A.5 and A.6. We consider the share of employment in five education categories in Appendix Table A.5, namely (i) missing/lower-than-secondary education (control mean: 18.4%), (ii) secondary education (57.1%), (iii) post-secondary education (9.20%), (iv) bachelor's degree (11.5%), and (v) master's degree or PhD (2.63%). Across these categories, we detect no systematic effects, with point estimates close to zero and standard errors between 0.005 and 0.01. We next study broad occupational shifts in Appendix Table A.6. We consider the share of (i) manual workers (control mean: 41.0%), (ii) lower-level white-collar workers (21.4%), and (iii) upper-level white-collar workers (8.44%), as well as a category of missing occupation information (29.3%). Again, we find no compositional shifts, with point estimates ranging between -0.003 and 0.004 (SEs around 0.1).

Capital-Labor Ratios Building up to our analysis of labor productivity, we study the capital-labor ratio in column (5) of Table 2. Consistent with the effects on log capital, we find positive and significant effects on log fixed assets per worker of 0.208 (SE 0.104) in the linear and of 0.244 (SE 0.120) in the quadratic specification. We find smaller or even negative effects for the placebos in the pre-period and at 100 employees as well as the predicted outcomes.

Labor Productivity and Total Factor Productivity We next analyze value added per worker as a labor productivity measure. In Column (7) of Table 2, we document positive point estimates for value added per worker at 0.062 (SE 0.043) in the linear specification and of 0.043 (SE 0.051) in the quadratic specification. In both cases, the 95% confidence interval includes zero and we can reject effects more negative than -0.022 or -0.056, respectively.

Why is that we find large capital increases but small output per worker effects? The data and basic production theory offer a two-step explanation. First, we find no effects on

[7]See Appendix Table A.4 for these specification as well as using separation rates as outcome.

total factor productivity, calculated as the firm-level Solow residual from a Cobb-Douglas production function with capital and labor, and an industry-level long-run labor income share. In column (7), we document point estimates of -0.03 (SE 0.064) and -0.04 (SE 0.075). In both cases, the confidence interval includes zero and, for the linear specification, ranges from -0.155 to 0.095 for the linear specification. Hence, we find no evidence that worker voice raises efficiency. We also find relatively wide standard errors and large (but not statistically significant) placebo effects, suggesting limited power of our analysis total factor productivity. Our findings are broadly in line with Jones et al. (2010) who find no relationship between measures of employee involvement and productivity in a survey of Finnish manufacturing firms (see also Ichniowski et al., 1997; Black and Lynch, 2001, for evidence for positive productivity effects of employee involvement).

Second, the capital share in our sample is not large, around 0.207 on average (control mean of Table 4 column (1)), which also represents the elasticity of value added per worker to the capital-labor ratio (holding efficiency constant). Hence, a 20% increase in fixed assets times a capital share of 0.2 would lead to a 4% predicted increase of value added per worker effect, provided no TFP effect (as we find), close to our actual point estimates of 4.3% and 6.2% .

5 Dividing the Pie: Wages, Rent Sharing, and Capital Income

We now study the distribution of the firm's value added between, and within, capital and labor.

5.1 Wages and Wage Structure

We start our analysis by studying wages. A central prediction of the hold-up view, and of wage bargaining models more generally, is that granting workers more authority will raise wages.

Total Labor Costs We study total labor costs at the firm level in Figure 6 and in column (1) of Table 3. Total labor costs are the sum of the wage bill and other labor costs such as payroll taxes and employee benefit programs. The econometric results reveal positive and statistically significant effects on log total labor costs of 0.087 (SE 0.033) in the linear specification and 0.087 (0.037) in the quadratic specification. We find no tantamount increase in the pre-period with a placebo estimate of 0.005 (SE 0.058); similarly, we find

substantially smaller effects at the placebo threshold of 100 employees (0.016, SE 0.027) and using predicted outcomes (0.002, SE 0.019).

Worker-Level Wages We now turn to the firm's average worker-level log wages, starting with raw wages, which we show in panel (b) of Figure 6. Here, we find a positive effect of 0.050 (SE 0.024) in the linear specification and 0.051 (SE 0.028) in the quadratic specification for the effect of shared governance on log worker-level wages (Table 3, Column (2)). We find no comparable increase in the pre-period placebo estimates (0.009, SE 0.038), at the placebo discontinuity (0.015, SE 0.018) or using predicted outcomes (-0.005, SE 0.012). We also study the effect on log median wages in column (5) and find comparable estimates to the ones for average worker-level log wages, with point estimates of 0.044 (SE 0.023) and 0.042 (SE 0.028) for the linear and quadratic specifications, respectively.

Are Shared Governance Firms High-Wage Firms, or Do They Employ High-Wage Workers? What explains the positive effects of shared governance on raw average wages? Shared governance firms could either be high-wage firms, i.e., pay higher wage premia. Or, shared governance workforces may consist of high-wage workers, who would earn higher wages no matter their workplace due to, for example, higher skill. That is, the wage effects could be driven by firms' shifting wage policies, or may arise from worker composition (although our analysis of basic skill proxies in Section 4.2 did not indicate such effects). We now separate the wage effects into whether shared governance firms become high wage firms, or whether they hire high-wage workers. We use Abowd, Kramarz, and Margolis (1999) regression specifications on worker-level wages, using our matched employer-employee panel data with firm fixed effects (denoting composition adjusted firm pay premia) and worker fixed effects (capturing the permanent earnings potential of a worker) and cubic controls for potential experience interacted with education groups. We estimate AKM specifications in rolling three-year windows and use observations from t, $t+1$, and $t+2$ to calculate outcomes for treatment assignment based on employment in t.[8]

Worker Selection: AKM Worker Effects To study the role of workforce composition in the raw wage effects, we report effects on AKM worker fixed effects in column (3) of Table 3, where we document a precisely estimated positive effect of 0.005 (SE 0.004) in the linear specification. We thus find evidence that shared governance leads firms to attract or retain

[8]A caveat to our current analysis is that the AKM results are currently based on data up to 2004 due to computational reasons and will be updated with the results for the whole sample in the next days.

workers with a higher permanent earnings capacity as proxied for by their AKM worker effect.

Isolating Firm Pay Premia: AKM Firm Effects The firm fixed effects are a simple estimate of the pay premia the firm extends to all employees irrespectively of their permanent earnings potential. They can represent differences in bargaining power,

We report effects on AKM firm pay premia in column (4) of Table 3 and document a small positive effect of 0.011 (SE 0.010) in the linear specification and 0.015 (SE 0.19) in the quadratic specification. Our results are thus consistent with small positive effects and the 95% confidence interval for the linear specification ranges from -0.013 to 0.041.

Together, our AKM effects appear inconsistent with the wage premium in the unadjusted worker-level wage effects, although we note that the *sum* of the worker and firm effects lie well within the confidence interval of the raw wage effect in column (3). Moreover, we suspect that the sample differences (our firm AKM effects are only estimated in a fraction of the sample, namely a connected set [and moreover only for a subset of sample years due to estimation speed ahead of the conference deadline.]

Rent Sharing The wage effects could be unconditional pay premia extracted similarly across all firms. Or, they may reflect workers extracting a larger share of the value-added pie in a bargaining setting. In fact, the workhorse model of hold-up assumes split-the-surplus rules like Nash bargaining, by which the pass-through of productivity into wages would identify the bargaining parameter (as in, e.g., Jäger et al., 2019) that is hypothesized to increase following increases in worker authority in corporate decisions (Manning, 1987).

To study for potential shifts in the wage setting process indicative of higher worker bargaining power in wage setting, we study effects on rent sharing, the cross-sectional relationship between firm-level wages and productivity, using the typical log-log specification. In a first step, visualize our results by estimating simple rent-sharing specifications relating $\ln(w)_{it}$ and $\ln(\text{VA}_{it}/N_{it})$ within bins of 20 employees, i.e., [90,110], ..., [190,210]. As we show in Figure 6 panel (d), there is no discernible change in rent sharing at the policy discontinuity. The levels of rent sharing are relatively high below the discontinuity with rent-sharing elasticities of about 0.3.

To further study rent sharing, we interact the right-hand side of specification (1) with log value-added per worker, and study whether productivity differences have a larger or

smaller effect on log wages in firms affected by the shared governance reform:

$$\begin{aligned}\ln(w)_{it} = \alpha + \beta_1 \mathbb{1}[N_{it} \geq 150] + \beta_2 \cdot (N_{it} - 150) + \beta_3 \mathbb{1}[N_{it} \geq 150](N_{it} - 150) \\ + \rho_1 \mathbb{1}[N_{it} \geq 150] \cdot \ln(\text{VA}_{it}/N_{it}) + \rho_2 \cdot (N_{it} - 150) \cdot \ln(\text{VA}_{it}/N_{it}) + \\ + \rho_3 \mathbb{1}[N_{it} \geq 150](N_{it} - 150) \cdot \ln(\text{VA}_{it}/N_{it}) + \gamma_t + \nu_{J(i)} + \epsilon_{it}.\end{aligned} \tag{2}$$

The coefficient of interest is ρ_1; positive values of ρ_1 indicate higher degrees of rent sharing in firms with worker representation following the reform. We report results in column (5) and find *negative* point estimates of -0.060 (SE 0.029) in the linear specification and of -0.026 (SE 0.019). Alas, we also find a sizable, and marginally significant effect in the pre-period placebo specification of -0.150 (0.086).

Overall, we therefore find no increases – if anything, decreases – in the degree to which workers benefit from productivity boosts at the firm. One implication is that the mechanism by which hold-up deters the fruits of capitalists' investment (higher output). Another substantive interpretation is that the wage effects appear to not be concentrated only in the most productive firms. These findings are consistent with previous empirical evidence for the absence of the wage channel hold-up (Card, Devicienti, and Maida, 2014; Jäger, Schoefer, and Heining, 2019).

Within-Firm Wage Structure Within-firm compression is one potential outcome of worker representation (as in the case of unions Freeman and Medoff, 1985; Western and Rosenfeld, 2011), in light of empirical evidence for firms sharing their rents unequally across worker types (Saez, Schoefer, and Seim, 2019; Drenik et al., 2020). We now directly study the within-firm ratio of log wages at the 75th to the 25th percentile, reporting results in column (6). We find negative point estimates of -0.036 (SE 0.022) and -0.035 (SE 0.025). This indicates a reduction by about 3.6% relative to a control mean of 0.78 (indicating that workers at the 75th percentile in firms at the cutoff have salaries 78 log points higher than those at the 25th percentile). That is, we find some but noisily estimated evidence consistent with pay compression within the firm in shared governance firms.

Executive Compensation We identify executives of a company (specifically using the board-level dataset, as often the highest level executives are not formally employees), and study executive compensation. Table 3, column (7), reports an effect on log executive compensation of -0.043 (SE 0.053) in the linear and of 0.000 (0.071) in the quadratic specification. We further express executive compensation as a share of a firm's total labor costs, we find a large reduction reduction with point estimates of -0.098 (SE 0.047) in the linear

specification and of -0.048 (SE 0.060) in the quadratic one. Our results thus point towards a reduction of the share of payroll to executives, but are less consistently estimated than the direct worker-level wage effects.

5.2 Capital Income

We now study the capital side of income, which has been the focus of the theoretical discussion of the effects of shared governance. For example, Jensen and Meckling (1979) argue that shared governance reduces profitability, and argue that, in line with the shareholder value paradigm (Friedman, 1970), capitalists would voluntarily adopt shared governance if it were profitable. By contrast, Freeman and Lazear (1995) and Levine and Tyson (1990) provide arguments for why individual capitalists may not do so even if advantageous as a macro institution.

Capital Share We start with firm-specific capital shares in column (1), calculated as one minus the wage bill divided by value added, in Table 4. We find no effects on the capital share with estimates of 0.002 (SE 0.010) in the linear specification and -0.004 (SE 0.012) in the quadratic specification, allowing us to rule out even small changes.

Capital Income We next study capital income in column (2), i.e., value added minus payroll, hence the cash flow accruing to capital, i.e., firm owners and creditors. If anything, we find an increase of 13.9 log points (SE 5.2) and 2.9 log points (SE 10.3) for the linear and quadratic specifications, respectively.

Profitability We next study net income (i.e., earnings after depreciation, interest, and taxation) as a measure of profitability in columns (3) and (4). This corresponds to the residual that the firm can retain or pay out as dividends. Column (3) normalizes by (the book value of) equity ("return on equity"), leading to small positive point estimates of 0.020 (SE 0.168) and 0.029 (0.189). Relative to the control mean of 0.30, these effects are small, and the confidence intervals include zero. Column (4) normalizes net income by revenue, with effects of -0.006 (SE 0.007) in the linear specification and 0.017 (SE 0.009) in the quadratic specification relative to a control mean of 0.01. However, recall that we found positive but noisily estimated effects on revenue, the denominator. Our estimates therefore cannot rule out small declines in profitability.

Dividend Payouts to Firm Owners The analysis of profit effects revealed no clear patterns for reductions in profitability, and, if anything, suggest an increase in cash flow.

However, net income measures the income that can potentially be paid out to owners, but it could also be retained and reinvested inside the firm. More importantly, firms with shared governance may actually pay out *less* than purely capital-controlled firms, and indeed our core results on positive investment effects may reflect yet another agency conflict akin to empire building on part of the workers Jensen and Meckling (1976).

We therefore directly study dividend payouts in logs in column (5), an indicator for positive dividends in column (6), and a zero-robust inverse hyperbolic sine transformation of a three-year moving average of dividend payouts in column (7). Throughout, we find positive point estimates on all measures of dividend payouts.

Finally, we study whether firms pay out any dividends in a given year. Column (5) of Table 4 reveals positive point estimates on the probability to pay out a dividend of 0.041 (SE 0.027) in the linear and of 0.029 (SE 0.03) in the quadratic specification.

Finally, in column (8) we revisit the negative point estimate on net income per revenue and find an increase in dividends as a fraction of revenue, a point estimate of 0.004 (SE 0.002), suggesting that the net income effect is not associated with reduced dividends.

Interest Expenditures Capital income, net of depreciation and taxes, can be divided between owners and lenders. In column (9) we study the effects on interest expenditures normalized by revenue. We find no evidence that shared governance firms pay lower or higher interest payments, with point estimates of 0.020 (SE 0.014) and 0.011 (0.008), which in principle could arise from labor-capital interactions through financial markets or frictions (Matsa, 2010; Chen, Kacperczyk, and Ortiz-Molina, 2011; Schoefer, 2015; Lin, Schmid, and Xuan, 2018; Matsa, 2018).

6 Conclusion

We have studied a 1991 reform in Finland that introduced the right to shared governance as a function of a firm's employment size, generating a discontinuity at 150 employees. Overall, we have found that the policy variation in shared governance has yielded a striking rejection of the canonical hold-up view of codetermination: while we have found moderate positive wage effects largely reflecting worker selection, we have found that shared governance *boosts* capital formation, all with no negative effects on profits and dividends. What may account for the positive investment effects?

One plausible candidate is that workers may not, as the hold-up review assumes, solely and primarily be involved in wage setting. In fact, in the Finnish setting, worker representatives are explicitly barred from formal participation in wage setting. In practice,

we suspect that through various indirect means, worker representations may well find wages to boost wages, say through facilitating appointments of labor-friendly managers (Bertrand and Schoar, 2003; He and le Maire, 2019), by making labor harder to replace, or by boosting labor's power more generally – consistent with the moderately positive effects on worker selection and on firms' pay premia. However, we have not found increases in rent-sharing elasticities; hence, shared governance does not appear to raise conventional bargaining power concepts.

Our evidence for positive capital effects is also consistent with richer views of labor relations. For example, if workers were involved in broader management decisions, as is in the case in Finland through board or management participation, models with bargaining over capital inputs would predict workers to call for higher capital formation (as in Manning, 1987; Jäger, Schoefer, and Heining, 2019). Similarly, efficient bargaining, if induced by shared governance, would lead to higher capital levels by overcoming hold-up (Crawford, 1988).

Finally, we note that the institution we study provides a relatively moderate boost of worker voice and codetermination, in fact giving *firms* the choice over which of three entities – on the board or in the management group – workers will be involved in. Workers are granted a statutory right, but need not take it up, perhaps permitting firms to bargain away shared governance in otherwise detrimental production. In fact, the policy explicitly permits the firm and its workforce to agree on alternative arrangements in lieu of this default institution, such as advisory councils. Relatedly, the share of seats to which workers have a statutory is limited to 20% – a low figure compared to, e.g., Germany where workers in firms with board-level codetermination make up between 33 and 50% of the supervisory board. As a cooperative institution with broader scope, shared governance may also differ in its effects from those of firm-level unionization in the US (see, e.g., Lee and Mas, 2012), which may represent a more adversarial institution and entail a more narrow focus on negotiation of wages and working conditions.

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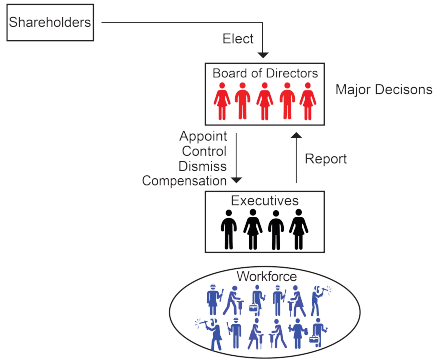
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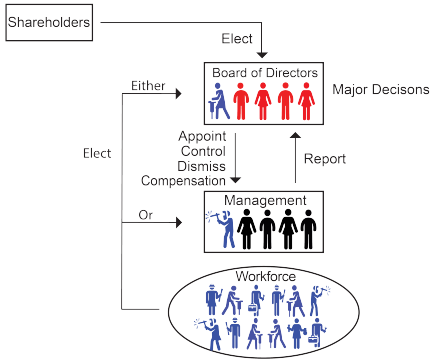
Figures

Figure 1: Corporate Governance and Worker Representation

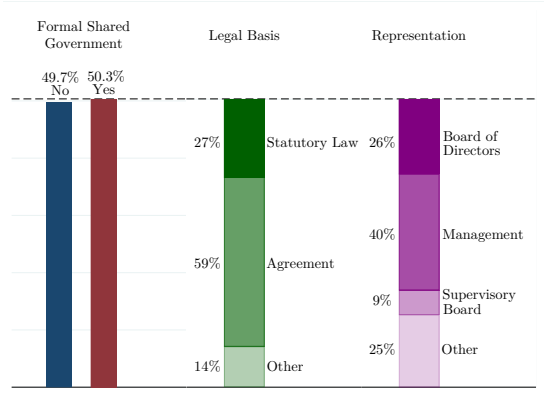
(a) Corporate Governance Without Worker Representation



(b) Corporate Governance With Statutory Worker Representation



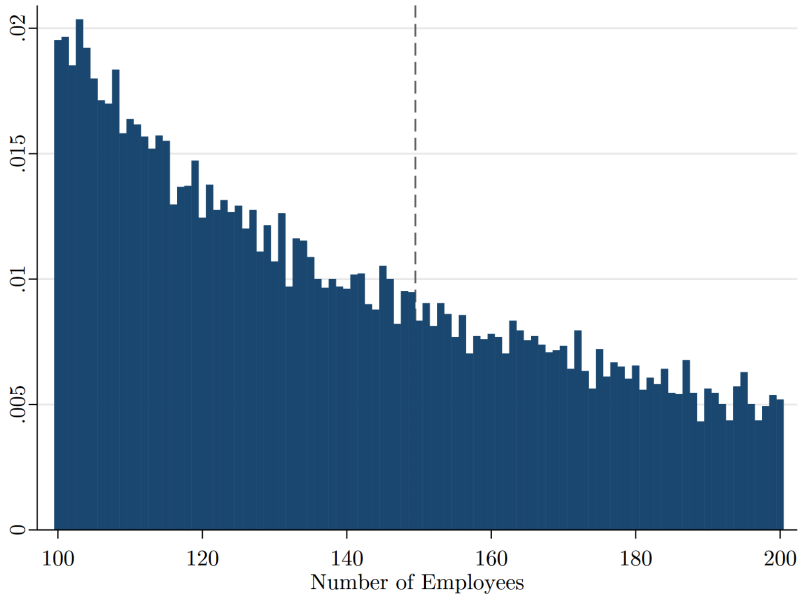
(c) Shared Governance in Firms With ≥ 150 Employees: Survey Evidence



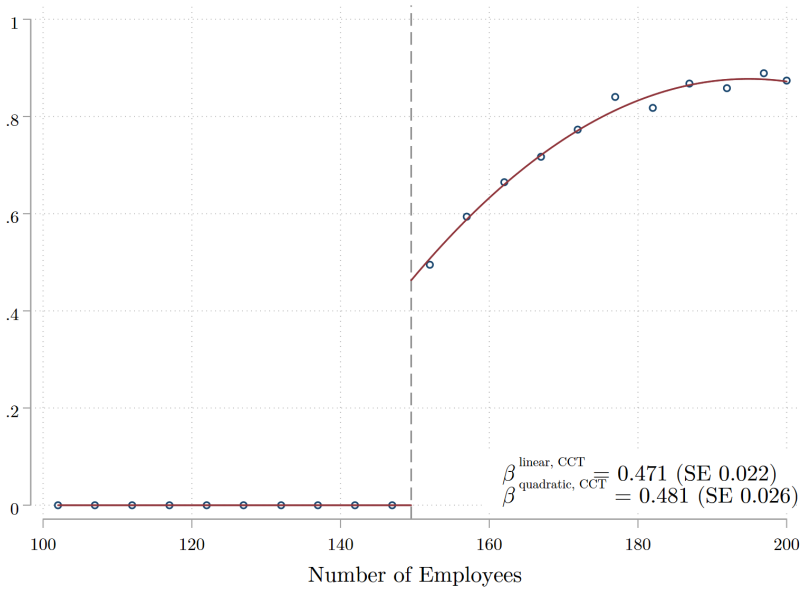
Note: The figure illustrates the governance structure of a Finnish firm with a unitary board structure. Panel (a) illustrates the governance structure without worker representation, which applied to firms before 1991 as well as to firms with fewer than 150 employees post-1991. Panel (b) illustrates the governance structure under the statutory provision of the codetermination law with 20% worker representation. We illustrate the cases where workers exercise their statutory right to elect representatives to either the board of directors or the firm's management group; under the statutory provisions, it is the firm's choice which of these bodies worker representatives are elected to. Here, we do not illustrate the less common case of a dual board structure and the option for worker representation on the supervisory board. Panel (c) reports results from a 2019 survey among union members (Teollisuuden palkansaajat, 2019). We focus on respondents in firms with at least 150 employees (the survey also had a few responses from workers in smaller firms). A small share of responses indicate that the respondent does not know the answer, in which case we omit it from our calculations. Examples of the "other" category for the legal basis include informal agreements or policies for worker representatives to have voice (but no vote) in board meetings. Examples of responses from the free-form "other" category for the body of representation include representation in two bodies, regular meetings between top management and worker representatives, and advisory boards.

Figure 2: Specification Checks for Regression Discontinuity Design

(a) Density of Firms' Employment



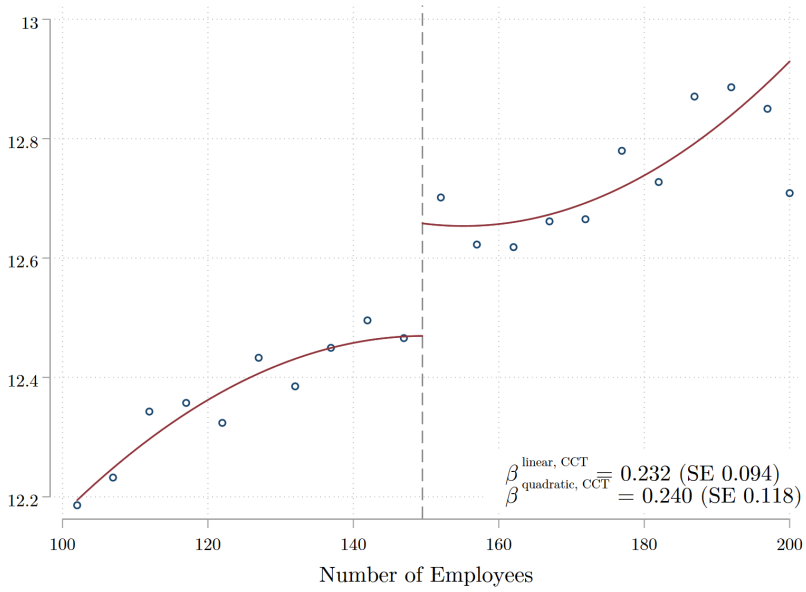
(b) Probability of Meeting Employee Threshold in Two Consecutive Years



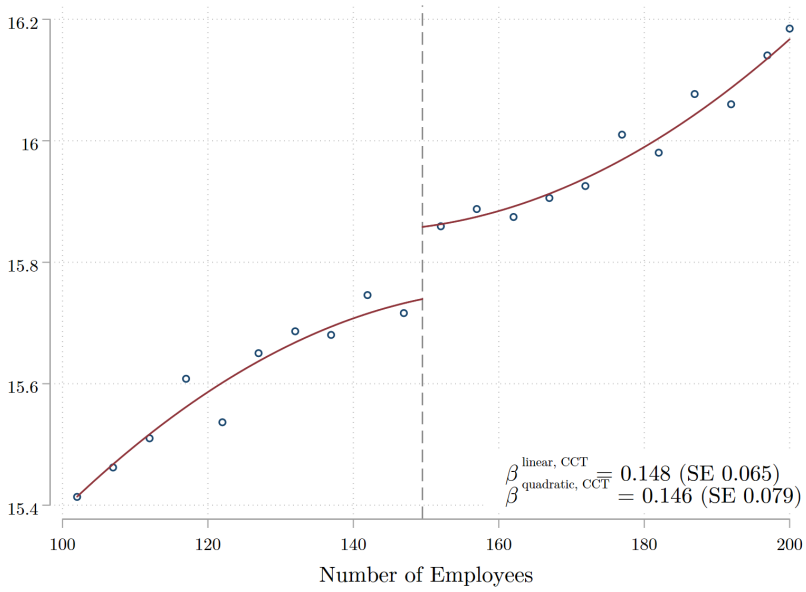
Note: Panel (a) plots the density of firms' employment around the 150 employee threshold in the post-reform period between 1992 and 2016. A McCrary (2008) test of the density at the 150 employee threshold does not reject continuity at the threshold ($p = 0.498$). Panel (b) plots an indicator function that is equal to one if a firm has crossed the 150 employee threshold in the past two years (as codetermination law binds when employment is at least 150 in two consecutive years).

Figure 3: Investment and Capital

(a) Log Investment



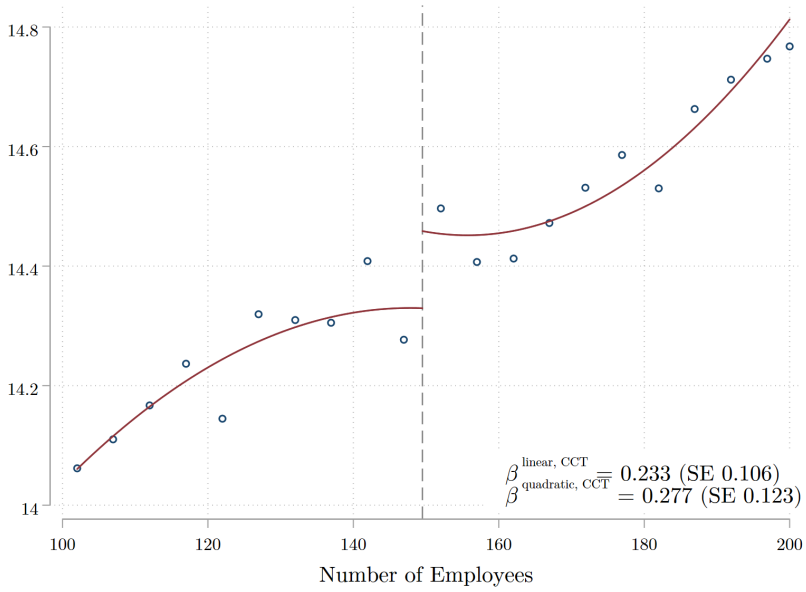
(b) Log Total Assets



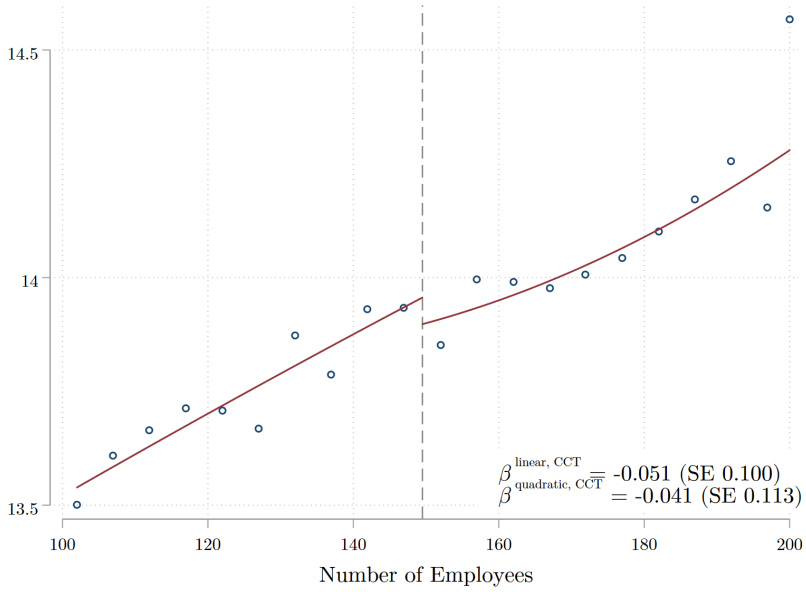
Note: The figures report binned scatter plot of the respective outcome variable plotted against employment. Firms with employment equal to or higher than 150 employees fall under the shared governance legislation giving workers a statutory right for participation in governance. The red lines report the predicted outcomes based on a quadratic specification. We report the corresponding estimates in Table 1.

Figure 4: Asset Composition: Fixed and Current Assets

(a) Log Fixed Assets



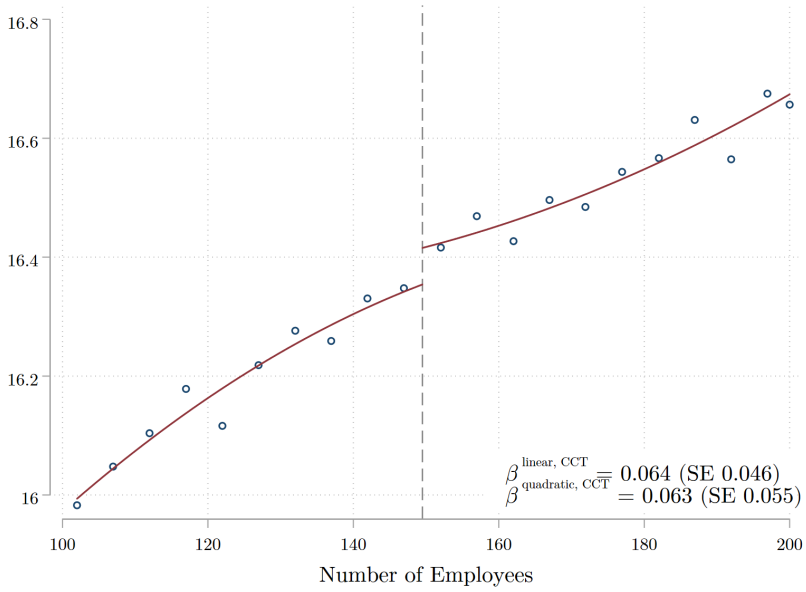
(b) Log Current Assets



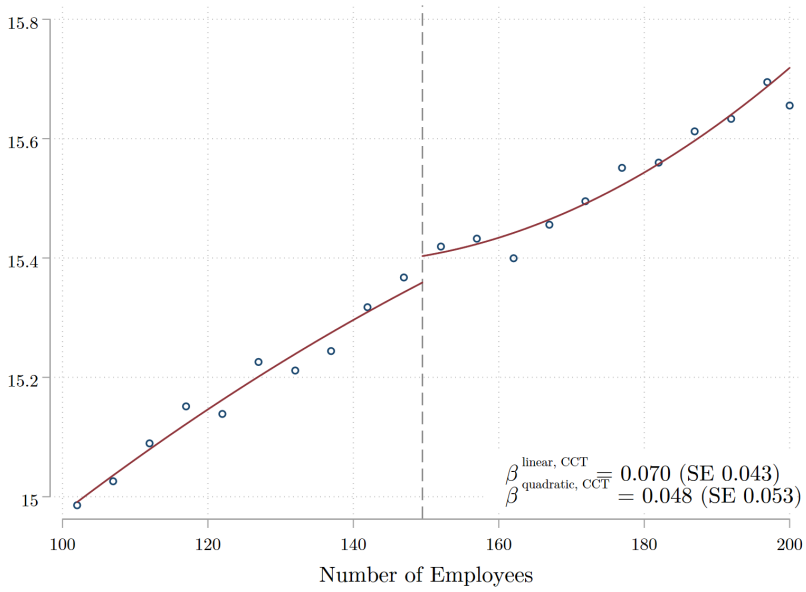
Note: The figures report binned scatter plot of the respective outcome variable plotted against employment. Firms with employment equal to or higher than 150 employees fall under the shared governance legislation giving workers a statutory right for participation in governance. The red lines report the predicted outcomes based on a quadratic specification. We report the corresponding estimates in Table 1.

Figure 5: Output

(a) Log Revenue



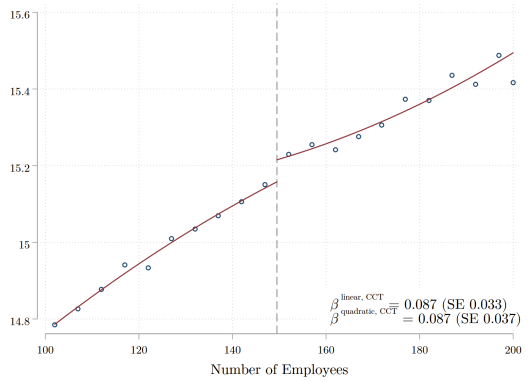
(b) Log Value Added



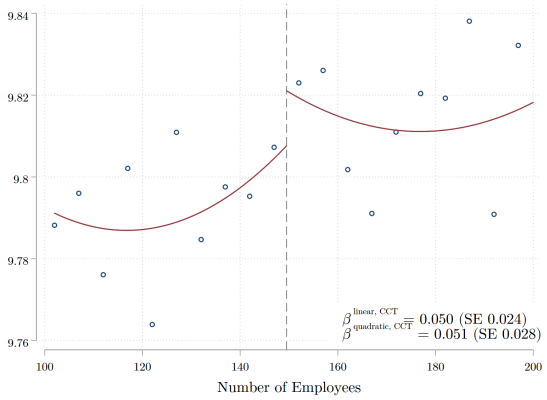
Note: The figures report binned scatter plot of the respective outcome variable plotted against employment. Firms with employment equal to or higher than 150 employees fall under the shared governance legislation giving workers a statutory right for participation in governance. The red lines report the predicted outcomes based on a quadratic specification. We report the corresponding estimates in Table 2.

Figure 6: Labor Income

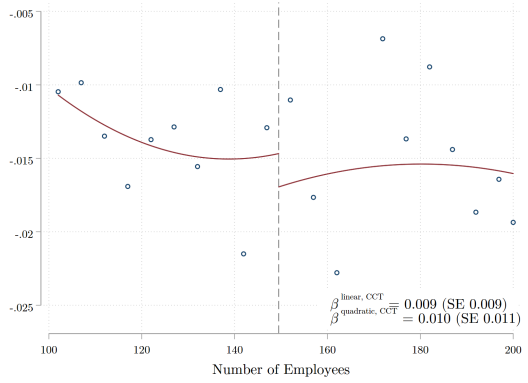
(a) Log Total Labor Costs



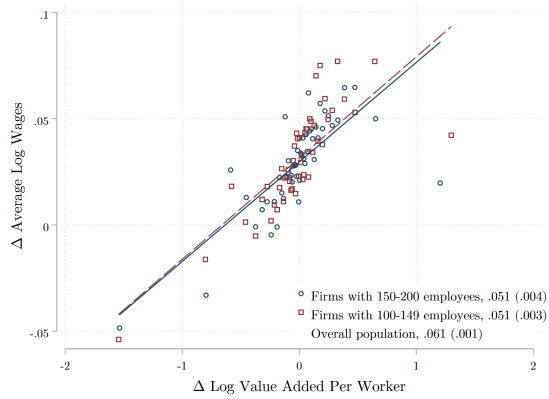
(b) Log Wage



(c) Firm AKM FE



(d) Rent Sharing Elasticity



Note: The figures report binned scatter plot of the respective outcome variable plotted against employment. Firms with employment equal to or higher than 150 employees fall under the shared governance legislation giving workers a statutory right for participation in governance. The red lines report the predicted outcomes based on a quadratic specification. Panel (d) shows the relation between yearly increase in log value added per worker and average log wages for firms with and without the right of employee representation. We report the corresponding estimates in Table 3.

Tables

Table 1: Investment and Capital

	(1) Log Investment	(2) $\mathbb{1}$(Investment > 0)	(3) arcsinh(Inv.)	(4) Log Total Assets	(5) Log Fixed Assets	(6) Log Current Assets
Panel A: Treatment Specification (1992 - 2016)						
Linear Fit	0.232**	0.014	0.169*	0.148**	0.233**	-0.051
	(0.094)	(0.015)	(0.087)	(0.065)	(0.106)	(0.100)
Bandwidth	[111, 172]	[124, 196]	[113, 173]	[114, 175]	[118, 170]	[110, 186]
Effective Observations	12,147	12,528	12,874	13,098	10,947	11,530
Quadratic Fit	0.240**	0.014	0.218**	0.146*	0.277**	-0.041
	(0.118)	(0.017)	(0.102)	(0.079)	(0.123)	(0.113)
Bandwidth	[113, 179]	[107, 200]	[105, 179]	[109, 183]	[110, 177]	[100, 192]
Effective Observations	12,156	18,347	15,987	15,916	14,544	14,707
Control Mean	12.48	0.89	12.58	15.73	14.34	13.93
Total Observations	63,443	72,127	69,497	72,847	70,698	51,958
Panel B: Placebo Specifications (Linear Fit)						
Pre-Reform Period	—	—	—	-0.032	-0.083	-0.032
(1988 - 1991)	—	—	—	(0.137)	(0.150)	(0.188)
Placebo Discontinuity	-0.031	0.007	-0.021	0.037	0.070	0.036
(100 Employees)	(0.068)	(0.010)	(0.061)	(0.053)	(0.074)	(0.072)
Predicted Outcomes	0.040	0.001	0.028	-0.001	0.008	-0.020
(Industry Averages)	(0.044)	(0.006)	(0.049)	(0.052)	(0.064)	(0.089)

Note: The table reports regression discontinuity specifications for the effect of having more than 150 employees—the legal threshold at which workers have a right for shared governance. The top panel reports local linear and quadratic specifications based on data from 1992 onwards, when the shared governance law has been active. We report three placebo specifications in the bottom panel. First, we report placebo specifications during the pre-1991 period before the law became active; the respective entries are empty ("—") when pre-reform data are not available. Next, we report placebo estimates for a (counterfactual) policy discontinuity at 100 employees. Finally, we assess the extent to which potential industry composition changes affect our estimates and use the leave-out mean of the outcome variable at the industry (NACE Level 1)-year level as outcome variable. Across all specifications, the reported coefficients and standard errors are based on the robust, bias-corrected procedure in Calonico, Cattaneo, and Titiunik (2014) with standard errors clustered at the firm level and industry and year fixed effects. We further show results for log investment and log total assets in Figure 3 and for log fixed and current assets in Figure 4. We show additional specifications in Appendix Table A.1.

Table 2: Output and Productivity

	(1) Log Revenue	(2) Log Value Added	(3) Log Materials	(4) Log Years Tenure	(5) Log Fixed Assets per Worker	(6) Log Value Added per Worker	(7) Log Total Factor Productivity
Panel A: Treatment Specification (1992 - 2016)							
Linear Fit	0.064	0.070	0.107	0.029	0.208**	0.062	-0.030
	(0.046)	(0.043)	(0.110)	(0.034)	(0.104)	(0.043)	(0.064)
Bandwidth	[113, 182]	[115, 169]	[120, 182]	[106, 186]	[117, 171]	[118, 169]	[128, 182]
Effective Observations	14,348	11,746	9,114	18,436	11,537	10,553	9,773
Quadratic Fit	0.063	0.048	0.110	0.068	0.244**	0.043	-0.040
	(0.055)	(0.053)	(0.119)	(0.042)	(0.120)	(0.051)	(0.075)
Bandwidth	[103, 186]	[119, 178]	[98, 194]	[107, 180]	[111, 178]	[119, 179]	[120, 196]
Effective Observations	18,903	11,860	16,260	17,372	14,483	11,686	13,246
Control Mean	16.34	15.34	15.07	1.84	9.43	10.38	6.28
Total Observations	73,564	71,333	54,382	76,398	70,698	71,333	69,317
Panel B: Placebo Specifications (Linear Fit)							
Pre-Reform Period	-0.038	0.021	—	-0.111	-0.064	0.021	0.141
(1988 - 1991)	(0.094)	(0.084)	—	(0.084)	(0.142)	(0.080)	(0.118)
Placebo Discontinuity	0.054	0.045	0.006	0.046	0.061	0.041	-0.030
(100 Employees)	(0.044)	(0.035)	(0.079)	(0.029)	(0.073)	(0.033)	(0.048)
Predicted Outcomes	0.008	0.006	-0.001	-0.013	0.003	0.002	-0.013
(Industry Averages)	(0.040)	(0.026)	(0.079)	(0.024)	(0.063)	(0.025)	(0.050)

Note: The table reports regression discontinuity specifications for the effect of having more than 150 employees—the legal threshold at which workers have a right for shared governance. The top panel reports local linear and quadratic specifications based on data from 1992 onwards, when the shared governance law has been active. We report three placebo specifications in the bottom panel. First, we report placebo specifications during the pre-1991 period before the law became active; the respective entries are empty ("—") when pre-reform data are not available. Next, we report placebo estimates for a (counterfactual) policy discontinuity at 100 employees. Finally, we assess the extent to which potential industry composition changes affect our estimates and use the leave-out mean of the outcome variable at the industry (NACE Level 1)-year level as outcome variable. Across all specifications, the reported coefficients and standard errors are based on the robust, bias-corrected procedure in Calonico, Cattaneo, and Titiunik (2014) with standard errors clustered at the firm level and industry and year fixed effects. We further show results for log revenue and log total factor productivity in Figure 5. We show additional specifications in Appendix Table A.3.

Table 3: Labor Income

	(1) Log Total Labor Costs	(2) Log Wage	(3) Worker AKM FE	(4) Firm AKM FE	(5) Rent Sharing Elasticity	(6) Log p75/p25	(7) Log Executive Wages	(8) Log Share Executive Wages
Panel A: Treatment Specification (1992 - 2016)								
Linear Fit	0.087***	0.050**	0.026	0.009	-0.047*	-0.036*	-0.049	-0.100**
	(0.033)	(0.024)	(0.022)	(0.009)	(0.025)	(0.022)	(0.051)	(0.047)
Bandwidth	[113, 172]	[108, 173]	[117, 181]	[108, 170]	[108, 173]	[110, 172]	[113, 192]	[105, 179]
Effective Observations	13,281	15,539	13,813	14,997	15,539	14,850	8,643	9,212
Quadratic Fit	0.087**	0.051*	0.026	0.010	-0.016	-0.035	-0.005	-0.043
	(0.037)	(0.028)	(0.025)	(0.011)	(0.016)	(0.025)	(0.072)	(0.061)
Bandwidth	[102, 180]	[95, 182]	[104, 202]	[109, 180]	[95, 182]	[102, 182]	[119, 188]	[121, 192]
Effective Observations	18,758	23,163	21,147	16,492	23,163	19,349	7,400	7,358
Control Mean	15.13	9.80	-0.01	-0.017	9.80	0.78	11.31	-3.87
Total Observations	75,679	76,398	75,025	75,025	76,398	76,398	39,923	39,923
Panel B: Placebo Specifications (Linear Fit)								
Pre-Reform Period	0.005	0.009	-0.073	-0.004	—	0.050	—	—
(1988 - 1991)	(0.058)	(0.038)	(0.057)	(0.018)	—	(0.040)	—	—
Placebo Discontinuity	0.016	0.015	0.011	0.002	0.011	-0.027	0.090*	0.076*
(100 Employees)	(0.027)	(0.018)	(0.018)	(0.007)	(0.018)	(0.017)	(0.046)	(0.041)
Predicted Outcomes	0.001	-0.026	-0.003	-0.002	0.001	0.009	-0.017	-0.006
(Industry Averages)	(0.022)	(0.020)	(0.013)	(0.002)	(0.003)	(0.012)	(0.015)	(0.014)

Note: The table reports regression discontinuity specifications for the effect of having more than 150 employees—the legal threshold at which workers have a right for shared governance. The top panel reports local linear and quadratic specifications based on data from 1992 onwards, when the shared governance law has been active. We report three placebo specifications in the bottom panel. First, we report placebo specifications during the pre-1991 period before the law became active; the respective entries are empty ("—") when pre-reform data are not available. Next, we report placebo estimates for a (counterfactual) policy discontinuity at 100 employees. Finally, we assess the extent to which potential industry composition changes affect our estimates and use the leave-out mean of the outcome variable at the industry (NACE Level 1)-year level as outcome variable. Across all specifications, the reported coefficients and standard errors are based on the robust, bias-corrected procedure in Calonico, Cattaneo, and Titiunik (2014) with standard errors clustered at the firm level and industry and year fixed effects. We further show results for log total labor costs, log wage, firm AKM fixed effects and rent sharing elasticity in Figure 6. We show additional specifications in Appendix Table A.7.

Table 4: Capital Income

	(1) Capital Share	(2) Log Capital Income	(3) Net Income over Equity	(4) Net Income over Revenue	(5) Log Dividends	(6) $\mathbb{1}$(Dividends > 0)	(7) arcsinh(Dividends)	(8) Dividends over Revenue	(9) Interest Payments over Revenue
Panel A: Treatment Specification (1992 - 2016)									
Linear Fit	0.002	0.139*	-0.020	-0.006	0.159	0.041	0.226*	0.004*	0.001
	(0.010)	(0.079)	(0.168)	(0.007)	(0.116)	(0.027)	(0.118)	(0.002)	(0.001)
Bandwidth	[108, 181]	[112, 177]	[126, 176]	[114, 183]	[108, 178]	[112, 174]	[115, 176]	[106, 173]	[115, 185]
Effective Observations	15,909	11,491	9,712	14,064	5,762	13,710	6,179	14,504	14,091
Quadratic Fit	-0.004	0.029	-0.029	-0.017*	0.109	0.029	0.199	0.004	0.001
	(0.012)	(0.103)	(0.189)	(0.009)	(0.140)	(0.030)	(0.137)	(0.002)	(0.001)
Bandwidth	[119, 199]	[123, 182]	[114, 194]	[112, 179]	[112, 194]	[104, 194]	[112, 194]	[99, 189]	[99, 196]
Effective Observations	14,429	9,307	15,836	14,003	5,846	19,217	7,943	19,530	21,417
Control Mean	0.207	13.64	0.30	0.015	12.59	0.38	12.12	0.014	0.012
Total Observations	72,537	59,571	72,766	71,447	28,289	72,127	37,539	67,857	71,447
Panel B: Placebo Specifications (Linear Fit)									
Pre-Reform Period	-0.014	-0.160	-0.710	-0.023	—	—	—	—	0.008
(1988 - 1991)	(0.027)	(0.198)	(0.555)	(0.017)	—	—	—	—	(0.005)
Placebo Discontinuity	0.008	0.051	-0.017	0.023***	0.036	-0.013	0.042	-0.001	-0.002
(100 Employees)	(0.008)	(0.067)	(0.094)	(0.007)	(0.095)	(0.020)	(0.090)	(0.002)	(0.001)
Predicted Outcomes	0.003	-0.000	0.008	0.001	0.009	0.002	-0.006	0.000	-0.000
(Industry Averages)	(0.004)	(0.045)	(0.019)	(0.001)	(0.045)	(0.005)	(0.042)	(0.001)	(0.001)

Note: The table reports regression discontinuity specifications for the effect of having more than 150 employees—the legal threshold at which workers have a right for shared governance. The top panel reports local linear and quadratic specifications based on data from 1992 onwards, when the shared governance law has been active. We report three placebo specifications in the bottom panel. First, we report placebo specifications during the pre-1991 period before the law became active; the respective entries are empty ("—") when pre-reform data are not available. Next, we report placebo estimates for a (counterfactual) policy discontinuity at 100 employees. Finally, we assess the extent to which potential industry composition changes affect our estimates and use the leave-out mean of the outcome variable at the industry (NACE Level 1)-year level as outcome variable. Across all specifications, the reported coefficients and standard errors are based on the robust, bias-corrected procedure in Calonico, Cattaneo, and Titiunik (2014) with standard errors clustered at the firm level and industry and year fixed effects. We show additional specifications in Appendix Table A.8.

Online Appendix of:
Worker Voice and Shared Governance:
Evidence from a Reform in Finland

Jarkko Harju, Simon Jäger, and Benjamin Schoefer

Contents

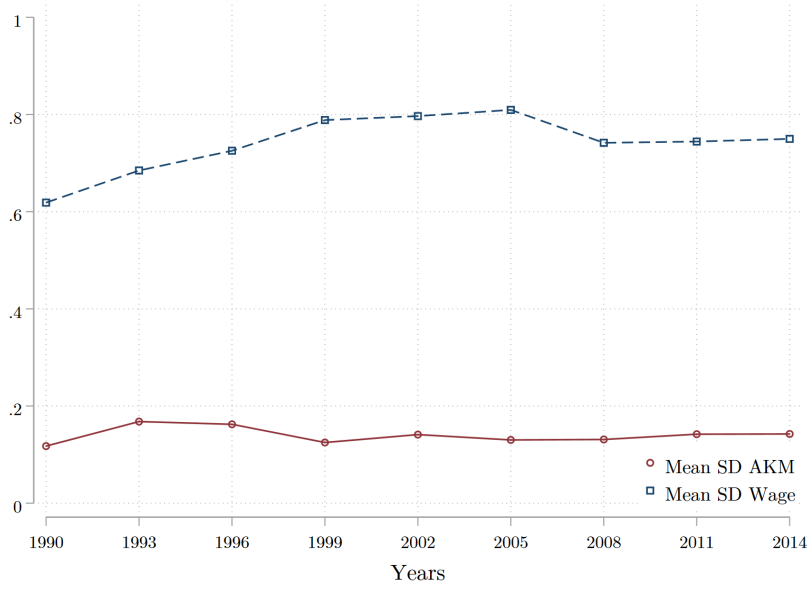
A Appendix Figures **38**

B Appendix Tables **44**

C Qualitative and Survey Evidence on Shared Governance in Finland **52**

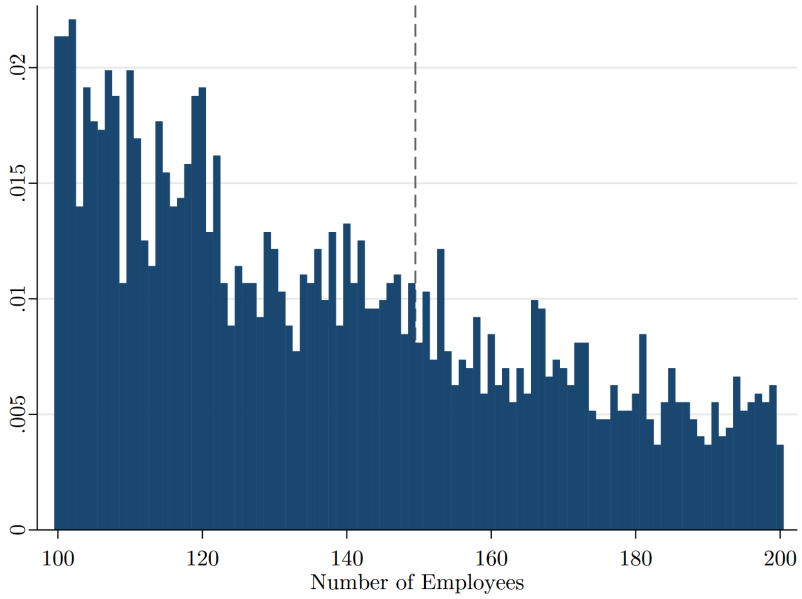
A Appendix Figures

Figure A.1: Standard Deviation of Log Wages and AKM Firm Effects Over Time



Note: The figure plots the standard deviation of log wages as well as of AKM firm effects over time. The AKM firm effects are estimated in three-year windows. The sample for this is estimation is based on the entire matched employer-employee data (rather than the firm size window for our main analysis).

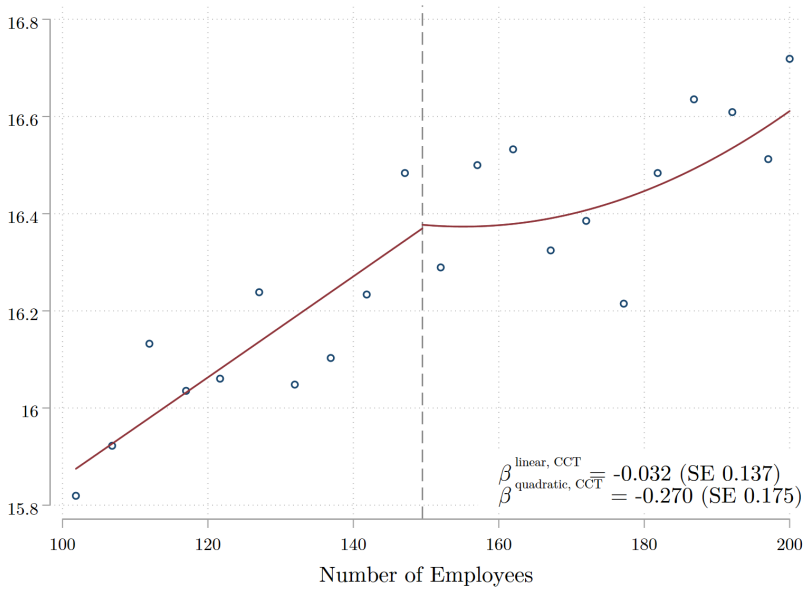
Figure A.2: Density of Firms' Employment (Pre-Reform Period)
(1988 to 1990, no policy discontinuity)



Note: The figures plot the density of firms' employment in the pre-reform period between 1988 and 1990, when no policy discontinuity at 150 existed. A McCrary (2008) test of the density at the 150 employee threshold does not reject continuity at the threshold ($p = 0.948$).

Figure A.3: Pre-Reform Period - Capital Income

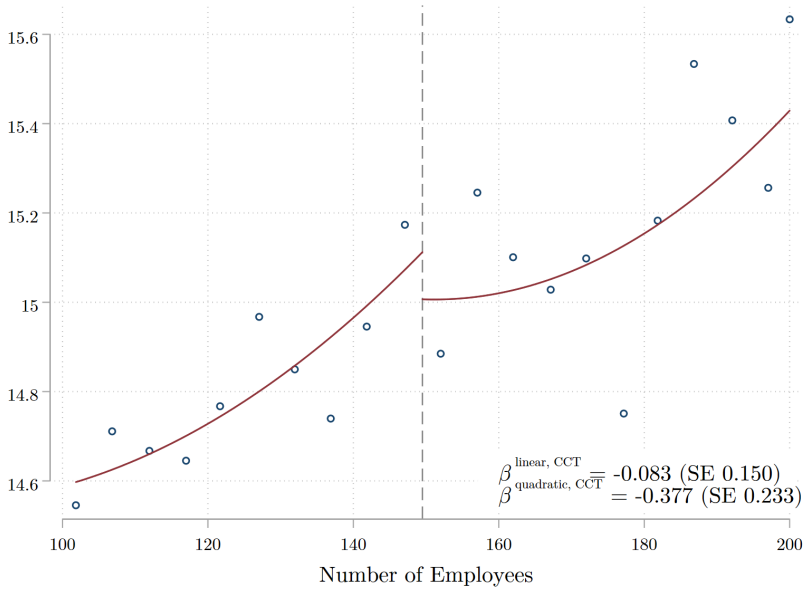
(a) Log Total Assets



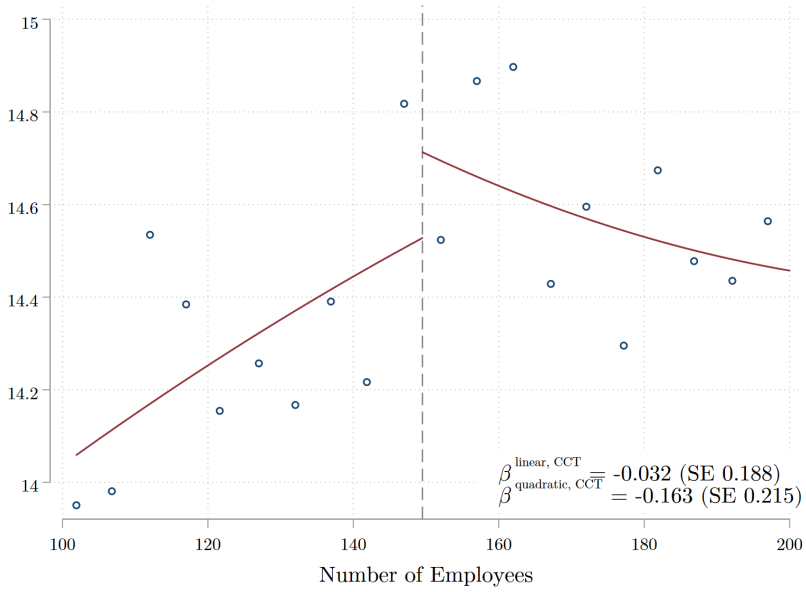
Note: The figures report binned scatter plot of the respective outcome variable plotted against employment. Firms with employment equal to or higher than 150 employees fall under the shared governance legislation giving workers a statutory right for participation in governance. The red lines report the predicted outcomes based on a quadratic specification. We report the corresponding estimates in Table 1.

Figure A.4: Pre-Reform Period - Capital Income

(a) Log Fixed Assets



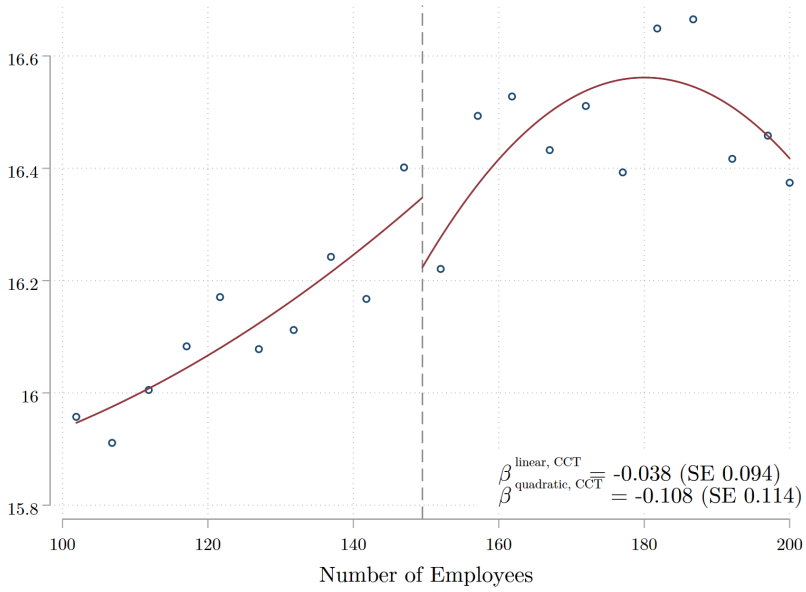
(b) Log Current Assets



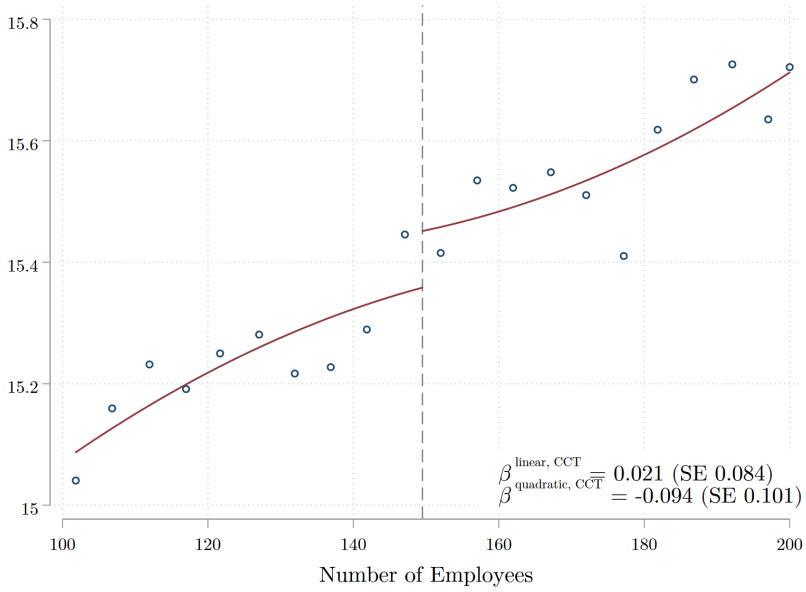
Note: The figures report binned scatter plot of the respective outcome variable plotted against employment. Firms with employment equal to or higher than 150 employees fall under the shared governance legislation giving workers a statutory right for participation in governance. The red lines report the predicted outcomes based on a quadratic specification. We report the corresponding estimates in Table 1.

Figure A.5: Pre-Reform Period - Output

(a) Log Revenue



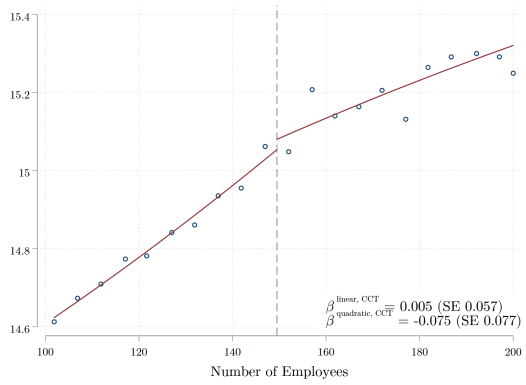
(b) Log Value Added



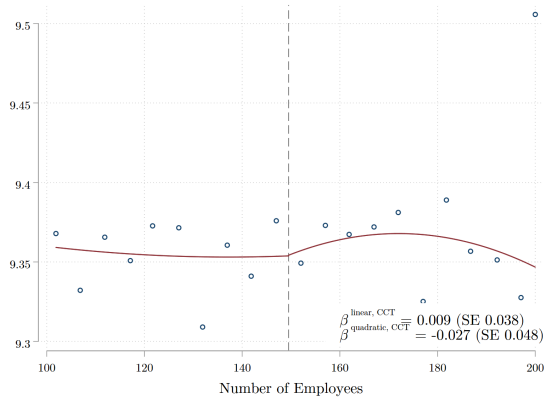
Note: The figures report binned scatter plot of the respective outcome variable plotted against employment. Firms with employment equal to or higher than 150 employees fall under the shared governance legislation giving workers a statutory right for participation in governance. The red lines report the predicted outcomes based on a quadratic specification. We report the corresponding estimates in Table 2.

Figure A.6: Pre-Reform Period - Output

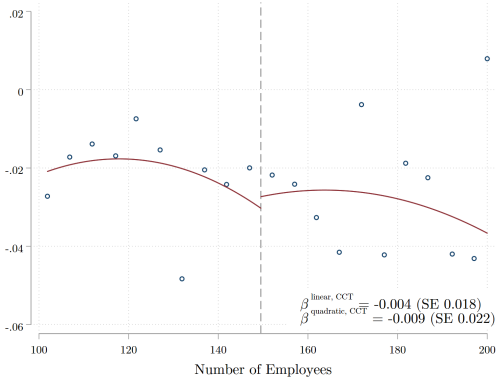
(a) Log Total Labor Costs



(b) Log Wage



(c) Firm AKM FE



Note: The figures report binned scatter plot of the respective outcome variable plotted against employment. Firms with employment equal to or higher than 150 employees fall under the shared governance legislation giving workers a statutory right for participation in governance. The red lines report the predicted outcomes based on a quadratic specification. We report the corresponding estimates in Table 3.

B Appendix Tables

Table A.1: Investment and Capital Robustness

	(1) Log Investment	(2) $\mathbb{1}$(Investment > 0)	(3) arcsinh(Inv.)	(4) Log Total Assets	(5) Log Fixed Assets	(6) Log Current Assets
Fixed Bandwidth at 50 (Linear)	0.186**	0.009	0.147*	0.129*	0.165	-0.049
	(0.094)	(0.015)	(0.085)	(0.066)	(0.101)	(0.100)
Fixed Bandwidth at 50 (Quadratic)	0.207*	0.017	0.177*	0.144*	0.228*	-0.029
	(0.114)	(0.019)	(0.102)	(0.079)	(0.121)	(0.118)
No Controls	0.241**	0.020	0.183*	0.129	0.208	-0.114
	(0.103)	(0.017)	(0.102)	(0.082)	(0.127)	(0.138)
Industry-Year Fixed Effects	0.258**	0.004	0.204**	0.140**	0.260**	-0.024
	(0.100)	(0.019)	(0.093)	(0.070)	(0.115)	(0.102)

Note: The table reports regression discontinuity specifications for the effect of having more than 150 employees—the legal threshold at which workers have a right for shared governance. Here, we report additional specifications as robustness checks. While the main results used the optimal bandwidth choice based on Calonico, Cattaneo, and Titiunik (2014) , rows 1 and 2 report results based on a fixed bandwidth of 50 employees around the discontinuity. The next row reports the linear specification but without control variables. The last row reports the linear specification with industry-by-year controls. Across all specifications, standard errors clustered are at the firm level. We report the main results in Table 1.

Table A.2: Investment Decomposition

	(1) Share Machinery and Equipment	(2) Share Buildings and Structure	(3) Share Software
Panel A: Treatment Specification (1992 - 2016)			
Linear Fit	-0.004	-0.006	0.015
	(0.017)	(0.009)	(0.017)
Bandwidth	[115, 177]	[115, 188]	[122, 174]
Effective Observations	11,746	12,721	9,492
Quadratic Fit	-0.013	-0.009	0.021
	(0.020)	(0.012)	(0.019)
Bandwidth	[98, 182]	[113, 193]	[105, 181]
Effective Observations	18,029	13,982	14,930
Control Mean	0.726	0.075	0.20
Total Observations	63,543	63,543	63,543
Panel B: Placebo Specifications (Linear Fit)			
Pre-Reform Period	—	—	—
(1988 - 1991)	—	—	—
Placebo Discontinuity	-0.005	0.008	-0.002
(100 Employees)	(0.013)	(0.008)	(0.012)
Predicted Outcomes	0.001	-0.002	0.001
(Industry Averages)	(0.004)	(0.003)	(0.004)

Note: The table reports regression discontinuity specifications for the effect of having more than 150 employees—the legal threshold at which workers have a right for shared governance. The top panel reports local linear and quadratic specifications based on data from 1992 onwards, when the shared governance law has been active. We report three placebo specifications in the bottom panel. First, we report placebo specifications during the pre-1991 period before the law became active; the respective entries are empty ("—") when pre-reform data are not available. Next, we report placebo estimates for a (counterfactual) policy discontinuity at 100 employees. Finally, we assess the extent to which potential industry composition changes affect our estimates and use the leave-out mean of the outcome variable at the industry (NACE Level 1)-year level as outcome variable. Across all specifications, the reported coefficients and standard errors are based on the robust, bias-corrected procedure in Calonico, Cattaneo, and Titiunik (2014) with standard errors clustered at the firm level and industry and year fixed effects. We report main estimates in Table 1.

Table A.3: Output and Productivity Robustness

	(1) Log Revenue	(2) Log Value Added	(3) Log Materials	(4) Log Years Tenure	(5) Log Fixed Assets per Worker	(6) Log Value Added per Worker	(7) Log Total Factor Productivity
Fixed Bandwidth at 50 (Linear)	0.058	0.043	0.107	0.034	0.158	0.044	0.021
	(0.048)	(0.041)	(0.106)	(0.035)	(0.099)	(0.040)	(0.059)
Fixed Bandwidth at 50 (Quadratic)	0.064	0.041	0.127	0.058	0.210*	0.039	-0.049
	(0.058)	(0.049)	(0.130)	(0.043)	(0.118)	(0.048)	(0.072)
No Controls	0.057	0.054	0.091	0.025	0.186	0.049	-0.088
	(0.060)	(0.051)	(0.145)	(0.042)	(0.124)	(0.051)	(0.083)
Industry-Year Fixed Effects	0.059	0.045	0.119	0.044	0.238**	0.043	-0.029
	(0.048)	(0.045)	(0.114)	(0.035)	(0.111)	(0.044)	(0.069)

Note: The table reports regression discontinuity specifications for the effect of having more than 150 employees—the legal threshold at which workers have a right for shared governance. Here, we report additional specifications as robustness checks. While the main results used the optimal bandwidth choice based on Calonico, Cattaneo, and Titiunik (2014) , rows 1 and 2 report results based on a fixed bandwidth of 50 employees around the discontinuity. The next row reports the linear specification but without control variables. The last row reports the linear specification with industry-by-year controls. Across all specifications, standard errors clustered are at the firm level. We report the main results in Table 2.

Table A.4: Separation Rates and Tenure

	(1) Separation Rate	(2) Log Avg. Tenure	(3) Log Avg. Tenure (1992-2004)	(4) Log Avg. Tenure (2005-2016)
Panel A: Treatment Specification (1992 - 2016)				
Linear Fit	-0.044	0.029	0.042	0.038
	(0.035)	(0.034)	(0.049)	(0.047)
Bandwidth	[119, 171]	[106, 186]	[118, 185]	[112, 179]
Effective Observations	11,233	18,436	6,476	8,053
Quadratic Fit	-0.066	0.068	0.039	0.105*
	(0.050)	(0.042)	(0.057)	(0.059)
Bandwidth	[110, 184]	[107, 180]	[108, 193]	[113, 176]
Effective Observations	15,847	17,372	8,779	7,537
Control Mean	0.24	7.74	7.77	7.72
Total Observations	74,077	76,398	35,208	41,190
Panel B: Placebo Specifications (Linear Fit)				
Pre-Reform Period	0.059	-0.111	—	—
(1988 - 1991)	(0.040)	(0.084)	—	—
Placebo Discontinuity	-0.006	0.046	0.026	0.065*
(100 Employees)	(0.015)	(0.029)	(0.038)	(0.038)
Predicted Outcomes	0.002	-0.013	-0.012	0.005
(Industry Averages)	(0.007)	(0.024)	(0.021)	(0.024)

Note: The table reports regression discontinuity specifications for the effect of having more than 150 employees—the legal threshold at which workers have a right for shared governance. The top panel reports local linear and quadratic specifications based on data from 1992 onwards, when the shared governance law has been active. We report three placebo specifications in the bottom panel. First, we report placebo specifications during the pre-1991 period before the law became active; the respective entries are empty ("—") when pre-reform data are not available. Next, we report placebo estimates for a (counterfactual) policy discontinuity at 100 employees. Finally, we assess the extent to which potential industry composition changes affect our estimates and use the leave-out mean of the outcome variable at the industry (NACE Level 1)-year level as outcome variable. Across all specifications, the reported coefficients and standard errors are based on the robust, bias-corrected procedure in Calonico, Cattaneo, and Titiunik (2014) with standard errors clustered at the firm level and industry and year fixed effects.

Table A.5: Workforce Composition - Education

	(1) Share Missing Educ.	(2) Share Secondary	(3) Share Post-Secondary	(4) Share Bachelor	(5) Share Master/PhD
Panel A: Treatment Specification (1992 - 2016)					
Linear Fit	0.009	-0.011	0.004	-0.000	0.003
	(0.006)	(0.010)	(0.005)	(0.006)	(0.008)
Bandwidth	[126, 187]	[122, 186]	[120, 196]	[116, 182]	[119, 175]
Effective Observations	7,779	8,616	9,760	9,375	7,732
Quadratic Fit	0.010	-0.011	0.004	-0.001	-0.002
	(0.007)	(0.011)	(0.005)	(0.007)	(0.009)
Bandwidth	[114, 199]	[112, 201]	[99, 204]	[105, 194]	[120, 187]
Effective Observations	11,279	11,847	15,749	12,948	8,657
Control Mean	0.145	0.47	0.124	0.153	0.111
Total Observations	50,186	51,693	50,699	51,053	46,366
Panel B: Placebo Specifications (Linear Fit)					
Pre-Reform Period	-0.044	-0.002	0.019	0.002	0.031**
(1988 - 1991)	(0.030)	(0.019)	(0.017)	(0.012)	(0.015)
Placebo Discontinuity	0.001	0.001	-0.002	-0.006	0.008
(100 Employees)	(0.004)	(0.008)	(0.004)	(0.005)	(0.007)
Predicted Outcomes	0.005	-0.000	0.002	-0.005	-0.002
(Industry Averages)	(0.004)	(0.006)	(0.003)	(0.003)	(0.004)

Note: The table reports regression discontinuity specifications for the effect of having more than 150 employees—the legal threshold at which workers have a right for shared governance. The top panel reports local linear and quadratic specifications based on data from 1992 onwards, when the shared governance law has been active. We report three placebo specifications in the bottom panel. First, we report placebo specifications during the pre-1991 period before the law became active; the respective entries are empty ("—") when pre-reform data are not available. Next, we report placebo estimates for a (counterfactual) policy discontinuity at 100 employees. Finally, we assess the extent to which potential industry composition changes affect our estimates and use the leave-out mean of the outcome variable at the industry (NACE Level 1)-year level as outcome variable. Across all specifications, the reported coefficients and standard errors are based on the robust, bias-corrected procedure in Calonico, Cattaneo, and Titiunik (2014) with standard errors clustered at the firm level and industry and year fixed effects.

Table A.6: Workforce Composition - Occupation

	(1) Share Manual	(2) Share Lower-Level	(3) Share Upper-Level	(4) Share Missing Occup.
Panel A: Treatment Specification (1992 - 2016)				
Linear Fit	0.003	0.004	0.001	-0.003
	(0.011)	(0.011)	(0.009)	(0.007)
Bandwidth	[107, 191]	[102, 187]	[113, 179]	[114, 180]
Effective Observations	11,884	13,684	9,839	9,793
Quadratic Fit	-0.001	-0.000	-0.001	-0.000
	(0.014)	(0.013)	(0.011)	(0.009)
Bandwidth	[104, 193]	[114, 204]	[106, 184]	[115, 188]
Effective Observations	12,790	11,590	12,123	10,276
Control Mean	0.394	0.283	0.159	0.175
Total Observations	49,382	51,411	49,399	51,474
Panel B: Placebo Specifications (Linear Fit)				
Pre-Reform Period	-0.061*	0.021	0.005	0.024
(1988 - 1991)	(0.034)	(0.026)	(0.021)	(0.020)
Placebo Discontinuity	0.009	-0.006	0.003	-0.001
(100 Employees)	(0.010)	(0.008)	(0.008)	(0.005)
Predicted Outcomes	0.005	-0.002	-0.001	-0.000
(Industry Averages)	(0.011)	(0.009)	(0.007)	(0.003)

Note: The table reports regression discontinuity specifications for the effect of having more than 150 employees—the legal threshold at which workers have a right for shared governance. The top panel reports local linear and quadratic specifications based on data from 1992 onwards, when the shared governance law has been active. We report three placebo specifications in the bottom panel. First, we report placebo specifications during the pre-1991 period before the law became active; the respective entries are empty ("—") when pre-reform data are not available. Next, we report placebo estimates for a (counterfactual) policy discontinuity at 100 employees. Finally, we assess the extent to which potential industry composition changes affect our estimates and use the leave-out mean of the outcome variable at the industry (NACE Level 1)-year level as outcome variable. Across all specifications, the reported coefficients and standard errors are based on the robust, bias-corrected procedure in Calonico, Cattaneo, and Titiunik (2014) with standard errors clustered at the firm level and industry and year fixed effects.

Table A.7: Labor Income Robustness

	(1) Log Total Labor Costs	(2) Log Wage	(3) Worker AKM FE	(4) Firm AKM FE	(5) Rent Sharing Elasticity	(6) Log p75/p25	(7) Log Executive Wages	(8) Log Share Executive Wages
Fixed Bandwidth at 50 (Linear)	0.069**	0.041*	0.024	0.002	-0.034*	-0.024	-0.041	-0.089*
	(0.032)	(0.024)	(0.022)	(0.008)	(0.020)	(0.021)	(0.053)	(0.047)
Fixed Bandwidth at 50 (Quadratic)	0.079**	0.053*	0.032	0.008	-0.018	-0.033	0.003	-0.059
	(0.039)	(0.029)	(0.027)	(0.011)	(0.017)	(0.026)	(0.067)	(0.059)
No Controls	0.076*	0.025	0.028	0.008	-0.036	-0.029	-0.073	-0.085*
	(0.039)	(0.030)	(0.026)	(0.009)	(0.022)	(0.024)	(0.052)	(0.051)
Industry-Year Fixed Effects	0.074**	0.051**	0.018	0.008	-0.043*	-0.035	-0.008	-0.090*
	(0.034)	(0.024)	(0.022)	(0.010)	(0.025)	(0.023)	(0.055)	(0.050)

Note: The table reports regression discontinuity specifications for the effect of having more than 150 employees—the legal threshold at which workers have a right for shared governance. Here, we report additional specifications as robustness checks. While the main results used the optimal bandwidth choice based on Calonico, Cattaneo, and Titiunik (2014) , rows 1 and 2 report results based on a fixed bandwidth of 50 employees around the discontinuity. The next row reports the linear specification but without control variables. The last row reports the linear specification with industry-by-year controls. Across all specifications, standard errors clustered are at the firm level. We report the main results in Table 3.

Table A.8: Capital Income Robustness

	(1) Capital Share	(2) Log Capital Income	(3) Net Income over Equity	(4) Net Income over Revenue	(5) Log Dividends	(6) $\mathbb{1}$(Dividends > 0)	(7) arcsinh(Dividends)	(8) Dividends over Revenue	(9) Interest Payments over Revenue
Fixed Bandwidth at 50 (Linear)	-0.001	0.116	0.005	-0.007	0.144	0.031	0.221*	0.003*	0.001
	(0.010)	(0.079)	(0.149)	(0.007)	(0.120)	(0.026)	(0.117)	(0.002)	(0.001)
Fixed Bandwidth at 50 (Quadratic)	-0.008	0.050	0.006	-0.013	0.065	0.021	0.149	0.002	0.001
	(0.012)	(0.094)	(0.194)	(0.009)	(0.144)	(0.032)	(0.122)	(0.002)	(0.001)
No Controls	0.006	0.168*	0.024	-0.006	0.108	0.040	0.154	0.004*	0.001
	(0.011)	(0.089)	(0.174)	(0.007)	(0.142)	(0.028)	(0.136)	(0.002)	(0.001)
Industry-Year Fixed Effects	-0.004	0.104	0.042	-0.011	0.072	0.040	0.139	0.003	0.019
	(0.010)	(0.080)	(0.200)	(0.008)	(0.124)	(0.030)	(0.110)	(0.002)	(0.016)

Note: The table reports regression discontinuity specifications for the effect of having more than 150 employees—the legal threshold at which workers have a right for shared governance. Here, we report additional specifications as robustness checks. While the main results used the optimal bandwidth choice based on Calonico, Cattaneo, and Titiunik (2014) , rows 1 and 2 report results based on a fixed bandwidth of 50 employees around the discontinuity. The next row reports the linear specification but without control variables. The last row reports the linear specification with industry-by-year controls. Across all specifications, standard errors clustered are at the firm level. We report the main results in Table 4.

C Qualitative and Survey Evidence on Shared Governance in Finland